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Edited for the
Academy of Political Science
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of Columbia University

Murd'rous Machiavel in France: A Post Mortem
DONALD R. KELLEY

Maoris and Muskets in New Zealand: Disruption of a War System
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The Application Clause of Article Five
PHILIP L. MARTIN

Reviews

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To Members of the Academy:

In accordance with provisions of the bylaws, notice is hereby given that the Board of Directors has fixed Monday, January 25, 1971, as the date for the Annual Corporate Meeting of the Academy of Political Science.

The meeting will be held in the office of The Academy of Political Science, Room 411, Fayerweather Hall, Columbia University, New York City, at 4:00 P.M.

Philip E. Mosely
Secretary

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Murd'rous Machiavel in France: A Post Mortem*

DONALD R. KELLEY
State University of New York at Binghamton

The better to betray, be false;
Lie, cheat, and try to hide the facts;
Always wear your sheep's disguise;
And never shun hypocritical acts.

Flatter and follow only the great;
Study Man, his plans and his deeds;
Worship not the soul but the State,
And God—when it suits your needs.

Demand peace when there is no peace;
Make friends with the Turk if you can;
Protect and arm the heretic;
But remember Machiavel is your man.

Of Providence, finally, don't bother to ask;
Just never discard your political mask.[1]

* Preliminary versions of this paper were offered at the Columbia University Seminar on Political and Social Thought on November 20, 1969 and at the American Historical Association Convention in Washington, December 29, 1969 in a session devoted to "Five Hundred Years of Machiavelli Interpretations." Special thanks to Felix Gilbert for his comments, private as well as public.

[1] Pour mieux trahir faire la chatemite,
Mentire, piper, deguiser verité
Couvrir le loup de fainte saincteté,
Sembler devot et n'estre qu'hypocrite,—

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In such accents Machiavelli seemed to speak to Christian readers, Catholic as well as Protestant, in the sixteenth century. Throughout Europe he created at once a shock of recognition and a wave of revulsion, and it is not surprising that a caricature of his views should have arisen. What needs explaining is not the caricature—we possess several equally gross distortions even today—but rather the extent to which it was broadcast, the uses to which it was put, and the ferocity with which it was attacked.

Unusual happenings suggest unusual explanations, and here is one of the most plausible. Contemporary reports to the contrary, Machiavelli did not really die in 1527. Half a century later, as Christopher Marlowe has told us, he was alive and well and living in France.[2] There he led a busier and certainly more successful career than he had ever had as a Florentine secretary and author. The evidence for his activities is quite as overwhelming as that for the contemporary phenomenon of witchcraft, and we have exactly the same obligation to accept it. Among other things, Machiavelli inspired the persecution of French Protestants by the Valois kings; he became a trusted adviser of the Guises and a constant companion of Catherine de' Medici and her children; he provided aid and comfort to the Jesuits and to the Catholic League as well as to the party of the *politiques;* and finally, so the story goes, the massacre of St. Bartholomew was a celebration held expressly in his honor. More generally, he helped to corrupt French

---

Flatter les grans, se renger a leur suite,
D'homme et de chair faindre une deité,
Sonner l'estat, et non la pieté,
Forger son Dieu au fond de sa marmite,—

Demander paix où paix estre ne peult,
Prest de coiffre le turban si l'on veut,
Nourrir soubz main et armer l'heretique,—

Pour Evangile Machiavel tenir,
De l'autremonde en rien se souvenir,
Sont les couleurs du masque Politique.

"Sonnet du Politique" from *De Jezabelis Angiae parrecidio varii poemata latina et gallica* (s.l.n.d.), cited by G. Ascoli, *La Grande-Bretagne devant l'opinion française* (Paris, 1927), I, 297.

[2] *The Jew of Malta*, l. 1-2:

Albeit the world think Machiavel is dead
Yet was his soul but flown beyond the Alps. . . .

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morals on every level and to infect French society with every Italianate vice from Ciceronianism to sodomy. It is this formidable bit of historical demonology, the posthumous career of Machiavelli in France, that is the subject of this paper.[3]

Although Machiavelli became the embodiment of a real "devil theory of history," there was nothing supernatural or even suspicious about his journey across the Alps. As the French translator remarked to his customers in 1544, "This Florentine merchant . . . has voluntarily left his own country to be received into yours. . . . Do not be so ungracious as to refuse him citizenship." From all appearances he was welcomed with open arms, or at least open minds. Within the next eight years the *Discourses on Livy*, the *Art of War*, and the *Prince* (in two different versions) appeared in French dress, while the *Florentine Histories*, though not translated until 1577, was read and cited in Italian. Within a few years, one of his admirers declared that he was a prophet honored more in France than in his own country.[4]

One reason for this, no doubt, was the somewhat extravagant advertising campaign that accompanied his works. According to the translator Jacques Gohory, for example, the *Discourses* offered "a mirror of universal history that could wonderfully instruct all manner of people." One of the first of those so instructed was Guillaume de la Perrière, who in 1555 displayed his own mirror, a "political mirror," which reflected the views of Machiavelli as

[3] Discussions of anti-Machiavellian literature are repetitive and not very critical. The best are G. Cardascia, "Machiavel et Jean Bodin," *Bibliothèque d'humanisme et renaissance*, III (1943), 129-67; Albert Cherel, *La Pensée de Machiavel en France* (Paris, 1935); and J. R. Charbonnel, *La Pensée italienne au XVIe siècle et le courant libertin* (Paris, 1919), 13 ff. Others include Charles Benoist, *Le Machiavélisme* (Paris, 1936), III; Giuliano Procacci, *Studi sulla fortuna del Machiavelli* (Rome, 1965); Andrea Sorrentino, *Storia dell'antimachiavellismo europeo* (Naples, 1936); Antonio Panella, *Gli Antimachiavellici* (Florence, 1943); Oreste Tommasini, *La Vita e gli scritti di Niccolò Machiavelli* (Turin, 1883), I; and, of course, the classic work of Meinecke, *Die Idee der Staatsräson in der neueren Geschichte* (Munich, 1924). And see now S. Mastellone, "Aspetti del anti-machiavellismo in Francia," *Il Pensiero politico* (1969), II, 376-416.

[4] Adolf Gerber, *Niccolò Machiavelli, Die Handschriften, Ausgaben und Ubersetzungen seiner Werke im 16. und 17. Jahrhundert* (Turin, 1962), III, 21; and the Italian sonnet prefacing the French translation of the *Discorsi* (1548) in Gerber, III, 23. I have relied heavily on this indispensable work.

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well as of Aristotle and so, hopefully, enhanced the image of the French monarchy.[5] The *Art of War*, too, was promoted vigorously. Jean Charrier suggested that his translation might teach Frenchmen not only how to acquire universal empire but perhaps even how to recover the holy land.[6] This book, too, was soon put to use, notably by Raymond de Fourquevaux, who published his highly derivative treatise on the practice of war just two years later.[7]

The appeal of the *Florentine Histories* was more limited, but this book had at least one practical application of interest. In their search for a "usable past," Protestant historians were willing to accept aid from any quarter. So it is not surprising, though it has passed unnoticed, that they should have applauded Machiavelli's criticism of the papacy. Flaccius Illyricus in his fundamental *Catalogue of the Witnesses of Truth* of 1556 even admitted Machiavelli to his honor roll of proto-Protestants, along with such disparate authors as Marsiglio of Padua, Jean Gerson, Lorenzo Valla, and Guillaume Budé.[8] Thus Machiavelli contributed, if only in a negative way, to the reconstruction of Lutheran "tradition."

Reform-minded Frenchmen, too, welcomed his assistance. In 1551 Charles Dumoulin published a curious and indeed unique work of scholarship, a Protestant gloss upon that most glossed of texts, the corpus of canon law; and the following year he used this material in a scathing attack upon the Roman curia. In both of these books, which constitute a thoroughgoing re-evaluation of ecclesiastical history, Dumoulin included Machiavelli among his authorities and ranked him far above such "wholly popish" historians as Flavio Biondo. "In the history which he wrote in the Tuscan language, especially in the first book," Dumoulin declared, "Nicholas Machiavellus depicted marvelously well the origin, rise, and expansion of the papacy, along with the ruses of the popes by which this was accomplished. At the beginning, as he said, the Roman pope had no authority except what was given through

[5] *Le Miroir politique* (Paris, 1567), f. 5[v]. In the introductory *epistre* there is an elaborate discussion of the terms *police* and *civilité*.

[6] Gerber, III, 28.

[7] *Instructions sur le faict de la guerre*, (ed.) G. Dickenson (London, 1954); originally published in 1548.

[8] *Catalogus testium veritatis* (Basel, 1556), 993.

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reverence for religion and doctrine." Dumoulin agreed that the real trouble began in the eighth century. The coming of the Lombards, he concluded, "led the pope to *farsi piu vivo*, as Machiavelli put it, that is, to take courage and to act." With the alliance between the papacy and the Carolingian Franks, the fortunes of Italy and France converged. Like Machiavelli, though from the opposite point of view, Dumoulin believed that the calamitous results of this encounter were still being felt in the sixteenth century.[9]

Then as now, of course, Machiavelli was normally identified with his *Prince*. In 1557 this work was honored with a place on the first papal index (not to be joined by his other books for another half century). Yet in France it seemed to raise few eyebrows at first. Machiavelli did use "somewhat intemperate language," admitted his translator Guillaume d'Auvergne, and he seemed "to loosen the ties of conscience."[10] But the blame for this should not be placed on Machiavelli, he argued; it belonged rather to "the naturally vicious law of the world" which required princes sometimes to exceed the normal bounds of morality. Machiavelli's purpose was simply to teach the powerful (the *seigneur Politiq*) how to conserve and augment their domain. What is more, in contrast to the many authors who described princes of superhuman perfection, he "accommodated the form of his precepts to experience." As Machiavelli's other translator, Guillaume Cappel, summed it up, "One might accuse him of portraying an excessively severe prince, and yet we may excuse him through the parallel of a good doctor, who looks not to his patient's pleasure but to his sickness, and so prescribes strong medicine."[11]

At this very time, there was a large and growing market for such strong medicine. There were French books on the theory and practice of politics, of course, but on the whole they tended to be either oppressively legalistic or blandly moralistic—either conventional lists of royal privileges or traditional "mirrors" of the prince. Closest in form to Machiavelli's work was the *Institution of*

[9] *Les Commentaires analytiques sur l'edit des petites dates* and *Annotationes ad jus canonicum*, in *Opera omnia* (Paris, 1681), IV, 464, 68.

[10] Gerber, III, 32.

[11] *Ibid.*, 34.

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the Prince compiled just a few years later by the great humanist Guillaume Budé.[12] Budé's "science politique," or "prudence civile," was loaded down with an impossible weight of moral, philosophical, and literary ornamentation. Closest in spirit to Machiavelli's work and also approximately contemporary was Claude de Seyssel's *Grand Monarchy of France*. Seyssel also emphasized experience, both diplomatic and military, and he framed his analyses in terms of what he called "natural and political reasons." His general purpose was "the conservation and augmentation of the French monarchy" through political and, if necessary, military means. Yet Seyssel very explicitly insisted that "la police" be tempered by religion and justice. Such, indeed, were the indispensable "bridles" restraining princely power.[13]

It is no wonder that Machiavelli spoke more directly to members of the next generation, who were beginning in a much more single-minded way to seek a "science" of politics. In 1567 Louis le Roy published a short history of political philosophy, thereby giving it a respectable pedigree and further definition.[14] He lamented its neglect and especially its divorce from experience, and although he was a disciple both of Budé and of Seyssel, he turned rather to Machiavelli as the most valuable modern supplement to Aristotle. Jean Bodin, who knew Machiavelli better than any other sixteenth century author, was still more enthusiastic. To him Machiavelli was "the first . . . for almost 1200 years after barbarism overwhelmed everything" to write intelligently about government.[15] Bodin's own ambition was to walk a little farther down Machiavelli's "untrodden path."

In other ways too, it would seem, Machiavelli had chosen a favorable time to settle in France. For it was precisely during the third

[12] *De l'Institution du prince* (Paris, 1547), 19. See the new edition and commentary in Claude Bontems *et al.*, *Le Prince dans la France des XVIe et XVIIe siècles* (Paris, 1965).

[13] *La Monarchie de France*, (ed.) Jacques Poujol (Paris, 1961), 115 ff.

[14] *De l'Origine, antiquité, progres, excellence, et utilité de l'art politique* (Paris, 1567). For his use of Machiavelli see his translation, *Les Politiques d'Aristote* (Paris, 1568), 614, 745, 788.

[15] *Methodus ad facilem historiarum cognitionem* (1566), in *Oeuvres philosophiques*, (ed.) Pierre Mesnard (Paris, 1951), 167. In his *Six livres de la république* (Paris, 1583), 939, Bodin criticized Machiavelli for his defense of "mixed government."

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quarter of the sixteenth century that men were beginning to distinguish and to define the public side of human behavior. In fact, even before Machiavelli became a public figure, French scholars were celebrating the arrival of the "homo politicus," in the words of François Baudouin.[16] At about the same time, in 1560, Etienne Pasquier published his own little dialogue on the "prince" and personified this point of view by calling his most important interlocutor "le politique";[17] fourteen years later, so would the author of the famous *Reveille-Matin*. It was during the 1560s, too, that *politique* was used as a highly charged, often derogatory way of characterizing anyone who placed public utility above religious principle. A later critic of *politiques* defined them as "those who refer all religion to policy or to government" and along with many others professed to detect in this practice the fine Italian hand of Machiavelli.[18]

Yet in the years between Pasquier's *Pour-parler du prince* and the *Reveille-Matin*, *politique* underwent a change in usage through that common process by which a contemptuous epithet becomes a proudly worn party label. Pasquier's friend Claude Fauchet argued that it was possible to be at once a Christian and a "bon politique," and many Protestants agreed. Thus the author of the *Reveille-Matin* caused his *politique* in the company of his colleague the *historiographe* to become converted to the true, which is to say the reformed, religion.[19] The basic reason for this shift in meaning, of course, was the massacre of St. Bartholomew. Long before this there had been a group of moderates (*moderatores*) inclined to a *via media*, but "Machiavelli's holiday" made some such program much more urgent. As one well-informed observer, and near victim of the massacre, wrote in the spring of 1574, "In Paris more than a thousand persons—nobles, magistrates, and merchants—have been

[16] *Commentarius de jurisprudentia Muciana* (1559) (Halle, 1729), 20. A similar discussion of "notre politic" appears in *Les Dialogues* (Paris, 1556), f. 40^{r}, by Baudouin's former student, Louis le Caron.

[17] *Pour-parler du prince*, in *Recherches de la France* (Paris, 1633), 993. Cf. *Le Reveille-Matin des François* (Edinburgh, 1574), attributed variously to Nicolas Barnaud, Hugues Doneau, and even (impossibly) to François Hotman.

[18] Leonardo Lessius, *De Providentia numinis et animi immortalitate* (1613), cited by Charbonnel, 33.

[19] *Documents concernant la vie et les oeuvres de Claude Fauchet*, (ed.) J. Espiner-Scott (Paris, 1938), 189.

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thrown in prison. They have taken the name of *politiques [politici]*, and . . . they demand the restoration of the ancient constitution through the convocation of the Estates General."[20] The author of this passage, François Hotman, was one of the most active propagandists both for this party and for the so-called "ancient constitution," which he had already celebrated in his notorious *Franco-Gallia*.

Hotman was no friend of Machiavelli, of course, nor of any of the "Italianized Frenchmen" who followed his doctrine. On the contrary he and the men of his party, who were "nouveaux reformateurs" as well as "bons politiques," according to one admirer, seized upon Machiavelli as the very embodiment of tyranny, atheism, and intolerance.[21] Machiavelli became not only a legend but a seed-bed of legends. He was regarded as the father-confessor to a whole tribe of frauds, fiends, and *fuorusciti*, and more especially to Catherine de' Medici, daughter of the man to whom he had dedicated his *Prince*. Stories were told that she took this book as her bible, that she brought up her children on it, that one of them, the future Henry III, carried it around in his pocket,[22] and that the massacre of Coligny and thousands of Huguenots was a direct application of the counsel given in the eighth chapter.

Such at least were the charges made by the Huguenot exiles in Switzerland, who began to sound the alarm against this monster. The *Reveille-Matin*, for example, adopted a frankly conspiratorial view, arguing that Machiavelli's evil genius had inspired official policy ever since the massacre of Vassy in 1562. The peace of St. Germain, concluded in the year of the first Machiavelli centennial, was represented as an offspring of the Cardinal of Lorraine—the "tiger of France," as Hotman had called him a decade earlier—and

[20] François Hotman to Wualter, 27 April 1574. Zürich, Zentralbibliothek, MSS, F. 39, f. 196[r].

[21] Nicolas Barnaud(?), *Le Miroir des François* (Paris, 1581), 291. Hotman's complaints against Machiavelli appear in a single letter to Wualter, 27 Sept. 1580, in Zürich, Staatsarchiv, E. II. 358, f. 79[v]; also in *Epistolae Hottomannorum* (Amsterdam, 1700).

[22] *De iusta Henrici tertii abdicatione e Francorum regno* (Lyon, 1591), II, 33; see also Franciscus Portus, *Ad Petri Carpentarii virulentam epistolam responsio* (s.l. 1573), 31-32. In September 1572, William of Hesse said the *Prince* was the Italian bible (*die welsche Bibel*); A. Kluckhohn (ed.), *Briefe Friedrich des Frommen* (Braunschweig, 1870), II, 496.

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of Catherine de' Medici, descended, in one vivid if implausible image, from a long line of Italian "buggers." Together, this "devil of a Guise" and his "whore" had created a "Sodom of horrible atheism" and the bloodbath of August 1572.[23] What was more shocking, even Frenchmen went along with this policy. The first president of the parlement of Paris, Christofle de Thou, allegedly expressed his approval by remarking, "He who knows not how to dissemble knows not how to rule." This maxim was not Machiavelli's, but it came from the next most likely source—Philip de Commines, who as everybody now knows was the "French Machiavelli."

The biggest blast "against Machiavel"—indeed the *primum in genere*—was that by Innocent Gentillet, who seemed almost literally to believe that Machiavelli had risen from the dead and that it was his mission to destroy Machiavelli once and for all. Under the reign of Henry II, Machiavelli's name was unknown in France, Gentillet wrote, but with the coming of the wars and the coming of the Italians, his doctrine began to infect every level of society.[24] While recognizing that "Doctor Machiavel" was the world's leading authority on tyranny, ranking even above Bartolus, Gentillet was not impressed with his method. Machiavelli had known no history, he had merely "fished stories" out of Livy, and in truth every useful thing in his writings had been stolen from other authors.

Gentillet's own method had even less to recommend it. What he did was to reduce Machiavelli's views, derived from the *Discourses* as well as the *Prince*, to a series of simple-minded "maxims" and

[23] *Le Reveille-Matin*, introductory poem on "La Paix Valoise"; see especially pp. 37, 71, 141. The author indicates his debt to Hotman's *Franco-Gallia* (p. 191). Etienne de la Boétie's *Discours de la servitude voluntaire* was first printed as part of the *Reveille-Matin*, though it was composed in about 1548. There is no reason to suppose, as does Joseph Barrère, *Estienne de la Boétie contre Nicolas Machiavel* (Bordeaux, 1908), that the work was a retort to the *Prince*.

[24] *Discours sur les moyens de bien gouverner et maintenir en bonne paix un royaume ou autre principauté . . . contre Nicolas Machiavel Florentine* ([Geneva], 1576), now in a critical edition (Geneva, 1968) with notes and introduction by C. Edward Rathé, who has published an analysis, "Innocent Gentillet and the First 'Anti-Machiavel,' " *Bibliothèque d'humanisme et renaissance*, XXVII (1965), 186-225, citing other literature, of which the most important is H. J. Schäfer, *Innocenz Gentillet* (Cologne, 1929).

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then to refute them by logical argument and historical example. He pressed his attack on three fronts: counsel, religion, and "pollicie." In the first place, Gentillet rejected the notion of absolute and arbitrary power in favor of a monarchy limited by fundamental laws, a faithful aristocracy, and an assembly of estates. Secondly, he attacked Machiavelli's "atheism" and defended not only the Christian religion but the Gallican church, which had long since found remedies for the usurpations of papal government so well described by Machiavelli himself. Finally, Gentillet condemned Machiavelli's prescriptions for cunning, cruelty, and hypocrisy as medicine worse than any disease; as a counter-remedy Gentillet suggested tyrannicide, resurrecting for this purpose the old arguments of Jean Petit. Such views inspired much of the revolutionary propaganda of these years, including the still more notorious *Defense of Liberty against Tyrants*, which likewise took "diametric opposition to the evil arts of Nicolas Machiavel."[25]

The Huguenot thesis, in short, was that the real guilt for France's plight in general and the massacre in particular lay neither with the tiger nor with the whore but with Machiavelli himself. He was also the moving spirit behind the Jesuit infiltration of France and, later, behind the Spanish League, argued Pasquier. Not only was he a policy-maker, he was also a perverter of youth. According to François de la Noue, he had converted a million to atheism; and curiously, a Catholic like Pierre Gregoire of Toulouse agreed that Machiavelli was a "most pernicious atheist."[26] In the face of such charges one hardly knows whether to applaud the sociological sophistication which permitted Huguenot critics to analyze the moral, social, and political corruption of France or to denounce the superstition that could attribute such conditions to the evil influence of a single man. (And *influence*, by the way, is a most appropriate term, being Italian in origin and having overtones both of pathology and of astrology, which happened to be a favorite pursuit of the Queen Mother.) In any case, we may conclude, a spectre was beginning to haunt Europe in the last quarter of the sixteenth

[25] *Vindiciae contra tyrannos* (1579), preface, in Latin edition of Machiavelli, *Princeps* (Leiden, 1648), 213 ff.

[26] La Noue, *Discours politiques et militaires*, (ed.) F. Sutcliffe (Geneva, 1960), 160; Gregoire, *De Republica* (Toulouse, 1582), XIII, 12.

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century—the spectre of "Machiavellism," for such was the term which men like Pasquier, Henri Estienne, and others were introducing at just this time.[27]

Thus Machiavelli's image was raised to the level not only of metaphor but of myth. He was identified quite literally by some with the *Zeitgeist*. "The misfortunes of our age," wrote Pasquier, "are such that in order to be counted an able man one must be a Machiavellian"; the phrase he coined was "il faut Machiavelizer."[28] Yet at the same time Pasquier could see far enough behind Machiavelli's political mask to realize the symbolic nature of his target. "There are the Machiavellisms of which the world was delivered before Machiavel was born," he admitted elsewhere; "and there be a great many Machiavels among us at this day, who never read his books."[29] Pasquier was thinking in particular of his own *bête-noir*, Loyola, but of course every man could select his own personal Machiavel to villify.

Had he been honest with himself, Pasquier might have added that there is a little bit of Machiavel in every man, even in a loyal Gallican like himself. Although he pronounced Machiavelli's books "fit to burn," in his own political work Pasquier aimed at "political demonstrations no less strict than those of mathematics for its professors."[30] Nor was Machiavelli's moral relativism unpalatable in itself. Nothing was more common among French political writers than the assumption, as La Popelinière wrote in his own embryonic "Anti-Machiavel," that the prince should rule "according to the humors of the men of his time rather than the laws of reason."[31] One of Pasquier's friends, Louis le Caron, told Henry IV—who hardly needed telling—that the public good (*l'utilité publique*) "changes often according to circumstances and events." Another of his friends, and a fellow Jesuit-hater, Pierre Ayrault, thought that it

[27] *Les Lettres*, XII, 9, in *Oeuvres* (Amsterdam, 1723), II, 348, also in the critical edition of Dorothy Thickett (Geneva, 1966), 321; and Estienne, *Principum monitrix musa* (Basel, 1590), 252.

[28] *Recherches*, 459.

[29] *The Jesuites Catechisme* (London, 1602), f. 64[r].

[30] *Recherches*, 22; cf. *Les Lettres*, IX, 7 (*Oeuvres*, II, 236).

[31] Bibliothèque Nationale, Manuscrits, Fonds français, vol. 20787, f. 19[v] ("Contre-Machiavel"); and yet he regarded Machiavelli the historian as "homme iudicieux," in *Histoire des histoires* (Paris, 1599), 104.

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might even override private morality. At any rate he distinguished between "things unjust in themselves, unjust in private terms, and unjust in public terms" and went on to suggest a formula that Machiavelli might have endorsed completely: "In republics one does not always consider what is just in itself, but what is relevant to the state, which maintains itself here in one way and there in another."[32]

Yet no French author, not even Commines or Machiavelli's imitator Jean de la Taille, could "match a villain" like Machiavelli. (The pun is contemporary and not of my making.)[33] His medicine was too strong for even his greatest admirers. Le Roy, for example, called him "an author without conscience and without religion." Even Bodin, who himself had a somewhat diabolical reputation among his countrymen, was of two minds about his works: after having praised him extravagantly in 1566, Bodin chose, a decade later, to decry the "tyrannical ruses . . . poured like a poison into his book *The Prince*."[34] And La Taille, who in 1573 offered a doggerel version of this book to Henry III, found it advisable to bowdlerize Machiavelli's arguments a bit.[35]

What made Machiavelli's views so indigestible to French readers? One reason was sheer and (if the phrase is not tautological) unadulterated xenophobia. From Petrarch to Machiavelli Italians had scorned the French as "barbarians"; now Frenchmen returned the compliment with interest. One particularly clear manifestation was murder "Italian style," as Henri Estienne described it in 1566. French gentlemen, who preferred duelling, did not even

[32] Le Caron, prefatory letter to Henry IV in Barnabé Brisson, *Code du Roy Henry III* (Paris, 1622), and Ayrault, *De l'Ordre et instruction judiciaire* (Paris, 1576), f. 29r.

[33] Edward Meyer, *Machiavelli and the Elizabethan Drama* (Weimar, 1897), 28. On Shakespeare's "murd'rous Machiavel," see p. 60.

[34] *Methodus*, 167. *Six livres de la République* (Cambridge, Mass., 1962), p. A70; preface, translation by K. D. McRae in his English edition.

[35] René Pintard, "Une adaptation de Machiavel au XVIe siècle: le 'Prince nécessaire' de Jean de la Taille," *Revue de littérature comparée*, XIII (1933), 385-402. It is amusing to see Catholics and Protestants using Machiavelli's name in vain against each other. In 1575, L'Estoile, *Journal de l'Estoile pour le règne de Henri III*, (ed.) L. R. Lefèvre (Paris, 1943), 89, characterized Hotman's critics as "corrumpus machiavélistes et Français italianisés," mentioning in particular Antoine Matharel, who himself, in his *Ad Franc. Hotmani Franco-Galliam . . . responsio* (Paris, 1575), 155, remarked that Hotman was even worse than Machiavelli.

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have a name for this unspeakable practice and so had to import a foreign term.[36] "Assassination" thus became a familiar part of French public life. But such outlandish conduct—unchivalric if not uncivilized—was symptomatic of a deeper disorder. Not only Protestants like Estienne, Gentillet, and Hotman, but Catholics like Pasquier and Ayrault seemed to detect a vast and growing Italian syndrome in the body politic. Whatever the etiology of the disease and whatever its remedy, the source of the contagion could be located precisely. "As among all nations in craft and subtiltie Italy beareth the name," declared Estienne in his diagnosis, "so in Italy Tuscane, and in Tuscane Florence excelleth."[37] And of all symbols to express this disease, of all scapegoats to blame it on, who was better qualified than Machiavelli?

But there was another and perhaps more fundamental antagonism, and this goes to the heart of Machiavelli's political methodology. According to Gentillet, there are two roads to knowledge, and Machiavelli was unable to keep his footing on either.[38] On what contemporaries would have called the "regressive road," the empirical approach moving from effects to cause, he showed his atheism, perversity, and general ignorance. On the deductive road, which moves from cause to effect, he lost his way entirely. He did not even realize, apparently, that its destination is not political science but mathematics. This fallacy above all—that is, the abstract, coldly logical, reductionist, and what we might call behaviorist method—alienated French intellectuals. Not even Machiavelli's supposed irreligion disturbed them more deeply; after all Bodin too acquired a reputation as an "atheist" and even a sorcerer, and yet he continued to be respected as a political philosopher.[39]

The fact is that Machiavelli and his French critics disagreed fundamentally about the very substance of history. For one thing French scholars, including *politiques* like Pasquier and Bodin, had

[36] *Apologie pour Hérodote*, (ed.) P. Ristelhuber (Paris, 1879), I, 353. Cf. Pasquier, *Recherches*, IX, 20; CSP For., Eliz., 1563, p. 92.

[37] *A mervaylous discourse upon the lyfe, deeds, and behaviours of Katherine de Medicis* (Heidelberg, 1575), 6. A sampling of popular anti-Italian literature may be found in *Journal de l'Estoile pour le règne de Henri III*, 90-103 *passim*.

[38] *Contre-Machiavel*, preface.

[39] *Scaligerana* . . . (Amsterdam, 1740), I, 500.

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an appreciation for ideological factors, which the limitations of Machiavelli's method hardly permitted him to acknowledge. They realized, of course, that some men were driven by material rather than spiritual motives, that for example there were "malcontents" as well as Huguenots struggling against the domination of the Guises; but they did not doubt that the wars were essentially religious in character. Contact with Calvinist and Jesuit ideologues intensified this attitude. On the other hand, they were aware of the force of tradition and the role of such institutions as the parlement and the university in fostering inertia. They had a sense of the complexity as well as the tragedy of history, and it is not surprising that they turned with renewed interest to the balanced kind of political analyses represented by Seyssel, whose work enjoyed a revival in the last quarter of the century.

Between Machiavelli and most French authors, moreover, were enormous differences in education and experience. Machiavelli's mind was shaped by the military and diplomatic services he performed for the Florentine Republic, while most Frenchmen followed academic and especially legal careers. François Baudouin had indeed referred to the "homo politicus," but he immediately added, "hoc est jurisconsultus." Most Frenchmen were cut off from power and from policy-making questions in general, and they were increasingly disillusioned about its uses and potential as one civil war succeeded another. Pasquier, listening to the harangues during the meeting of the Estates of Blois in 1588, seemed to be hearing only the "swan's song" of the monarchy; in general, he felt, men were losing control of their own fates.[40] These are perhaps some of the reasons why French scholars of this generation were concerned more with description than with causal analysis, more with the nature of institutions than with the laws of politics, more with the temper of society than with individual psychology. If their work was less probing than Machiavelli's, it was also more comprehensive; they preferred a net to a harpoon, as it were, in the reassessment of their past. It is not surprising that men like Pasquier should look upon history less as a store of vicarious experience to be used in future calculations than as a drama, an increasingly tragic drama, to be watched. In a sense history was retreating from politics and

[40] *Les Lettres*, XIII, 7 (*Oeuvres*, II, 378; [ed.] Thickett, 383).

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reverting to its moral function.

Yet there was a positive side to this. In general, French scholars had a much deeper sense of individuality and of historical differences. Machiavelli deliberately ignored questions of cultural tradition, specific social context, and institutional peculiarity, but Pasquier, Bodin, and Hotman, worlds apart as they might be on religious or political grounds, took such problems and indeed "national character" in general as the substance of history and of any possible science based on history. Because they were "barbarians" they could not—and by this time would not—connect their own traditions with ancient Rome. They could not even use Rome, as Machiavelli did, as a model of political experience. Such were the assumptions of Hotman's "anti-Tribonianist" arguments and Pasquier's reconstruction of French culture and of that comparative method developed by Bodin and other French jurists.[41]

What I am suggesting, in short, is that the anti-Machiavellian backlash in sixteenth-century France stemmed not only from offended political and religious sensibilities but also from a clash between different modes of perceiving reality—not only from a doctrinal *Kulturkampf* but from a fundamental, though not always fully articulated, *Methodenstreit*. Machiavelli frightened, while at the same time he fascinated, his French readers by that political mask which he never seemed to discard. Indeed, because of the brutality and cynicism of his political analyses he seemed to be, in an intellectual sense, more of a tyrant than his "prince." Now obviously, this contrast between Machiavelli as a hawkish policy-maker and his French critics as sophisticated and inhibited academic doves can be overdrawn, and perhaps I have overdrawn it. Yet, in a sense, these men brought it upon themselves by conjuring up this devil which modern scholarship has still not completely exorcised. For it is largely because of these worshipful French scholars that Machiavelli remains, at least in one of his reincarnations, the demon of power. Because of their needs and fears he became not only an "advocate of tyranny," he emerged as the bogeyman of the western world.

[41] See the remarks of Jean Moreau-Reibel, *Jean Bodin et le droit public comparé* (Paris, 1933), 53, and J. G. A. Pocock, *The Ancient Constitution and the Feudal Law* (Cambridge, 1957), especially chap. I.

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Maoris and Muskets in New Zealand: Disruption of a War System*

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The New Zealand Maoris live and work now in peace as citizens of a modern nation, but they are a people who, like some North American Indian groups and the Zulu of Africa, first became famous as warriors. A great deal has been written about Maori warfare. Particularly those aspects which seemed strange to European observers—cannibalism, the taking of heads, fighting in order to avenge verbal insults—have been richly and conspicuously documented, but copious materials on other aspects have also been made available by observers and students of Maori life in the two centuries since Captain Cook and other Europeans first landed in New Zealand. In this paper these materials are used to indicate that Maori warfare had an adaptive function within a system operating in pre-European times and to show that the effect of certain changes brought about through contacts

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with Europeans in New Zealand in the early nineteenth century was to disrupt this function and to make the war system maladaptive. In other words, our main point is to show a way in which maladaptations involving warfare may have their sources in adaptations in the past. Further, the materials on pre-European times in New Zealand allow us to call attention to something overlooked by many writers who consider revenge to be a main goal or cause of primitive warfare, namely, the possibility that important systemic connections exist between land use and fighting for revenge.

The Pre-European System: Warfare, Land Use, and Revenge

It is appropriate to deal here with the traditional setting of Maori warfare. The Maoris' ancestors, who came in their canoes from the small, tropical islands of eastern Polynesia, found in the temperate, continental land mass of New Zealand a new and challenging environment. At the time of Cook's arrival in 1769, the Maoris, after centuries of living in New Zealand and after having hunted the moa and other indigenous species of flightless birds to extinction, were still in the process of adapting to this environment and extending their exploitation of it.

Warfare was part of that process in the late eighteenth century. The roughly forty Maori tribes of New Zealand comprised at this time some 100,000 to 300,000 people but a considerably larger population could probably have been sustained even without the introduction of European technology. A New Zealand geographer, Kenneth B. Cumberland, has gone so far as to say that a million Maoris could have been supported "in view of the reserves of bird life, fish, forest fruit, and timber and of cultivable soil, and in view of the equipment at the disposal of the people, their skills, and their knowledge."[1] This is probably an exaggerated estimate, but the important point is that there still was unexploited or underexploited land where the Maoris could have hunted, fished, grown their sweet potatoes, or dug the fernroots which were a staple of their diet, and the question that presents itself is what was the role of warfare in moving people to such land.

[1] "Aotearoa Maori: New Zealand about 1780," *Geographical Review*, 39 (1949), 417.

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As long as growing tribes or subtribes were able to expand into virgin land adjoining areas already being used by them, the increase and spread of the Maori population from its small beginnings in the early period of settlement could take place peacefully.[2] However, as soon as there were groups whose territory adjoined only the territory of other groups rather than any unoccupied land suitable for exploitation by Maori tools and techniques, warfare could have a role. The process of expansion could have taken place among the kinship-organized, genealogy-conscious Maoris very much as it did among the Tiv of northern Nigeria, a people whose recognized technique for the expansion of land holdings was, as recorded by the anthropologist Paul Bohannan, a simple one:

> . . . always, when you make new farms, clear land towards that man whose land bounds yours, but who is more distantly related to you. When he objects, you are thus assured of the largest possible supporting group in any litigation, argument, or fight which may develop, since all the people who are more closely related to you than to him will come to your aid instead of his.[3]

The expansion of the Tiv in all directions was still going on during Bohannan's term of field work, and the kinds of opportunities that Bohannan had for observing the process of expansion are, of course, denied to the student of Maori society. Yet there is evidence that the process among the Maoris not only could have been but actually was similar in some significant respects. The influence of considerations of genealogical proximity in determining to whom aid would be given in Maori warfare is shown, for example, in a European trader's eyewitness account of fighting between two sections of the Nga Puhi tribe in 1837. The trader, J. S. Polack, noted that the Hokianga natives, arriving after the hostilities had begun, were sorely puzzled which of the two parties to join, as they were equally related to both.[4] There are, moreover, many examples in pre-European New Zealand of conquests

[2] This and the succeeding two paragraphs are taken, with some changes, from my "Expansion and Warfare among Swidden Agriculturalists," *American Anthropologist*, 63 (1961), 348-50.

[3] Bohannan, "The Migration and Expansion of the Tiv," *Africa*, 24 (1954), 5.

[4] Polack, *New Zealand* (London, 1838), II, 42.

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involving displacement of vanquished foes from their territories, and there are cases too of beaten and displaced groups which, in turn, became the vanquishers and displacers of yet other groups.[5] There is evidence, in other words, that among the Maoris a chain-reaction process of aggressive territorial expansion operated in which the more closely related people or groups supported each other against the less closely related and unrelated. In this process, the expansion of one group, by means of warfare, into the contiguous territory of another could lead the second group to expand into the contiguous territory of a third, and so forth until finally there would be displacement of a group having territory contiguous to unoccupied land.

At this point, we have already gone some way towards indicating the adaptive function of Maori warfare within a traditionally operating system, and we may be able to say that warfare had the function of maintaining the dispersion of people over the land and that this function was adaptive for the Maoris as a whole because it entailed a more extensive exploitation of the total New Zealand environment which, in turn, enabled the Maori population to continue to grow without the over-exploitation and degradation of particular localities. However, some important questions remain about the system within which the dispersion of people through warfare operated. When we talk about the functions of an activity like warfare, we are talking about certain kinds of regular consequences which it has. More specifically, in my use of the term *function* the consequences always involve the restoration of some systemic variable, like the degree of population dispersion, to a proper, advantageous, or acceptable state.[6] Without elaborating here on the reasons for using the term in this way, we may simply note that specifying the consequences of an activity still leaves the question of the motivations that people have for engaging in it and, further, the question of the extent to which these motivations themselves might be components of a system and might be

[5] See the citations in my *Maori Warfare* (Wellington, 1960), 110, 115.

[6] Andrew P. Vayda, "Hypotheses about Functions of War" in Morton Fried *et al. (eds.), War: The Anthropology of Armed Conflict and Aggression* (Garden City, N.Y., 1968), 85, 102-05; Paul W. Collins and Andrew P. Vayda, "Functional Analysis and Its Aims," *Australian and New Zealand Journal of Sociology*, 5 (1969), 153-56.

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operating in response to systemic forces rather than arbitrary or capricious ones.

In the course of centuries, Maori warriors have had, no doubt, a considerable variety of individual, situationally determined, and idiosyncratic motives for taking up arms; however, the two main recurrent motives of the Maoris for initiating war were to acquire land and to revenge themselves upon groups that committed offenses against them. The offenses provoking tribes or subtribes to seek vengeance include homicide or other violence either by physical or fancied magical means against members of the group (including women married to the offenders); trespass or poaching on food preserves claimed by the group; adultery with women of the group; theft of valuables; and insults to the group or, what amounts to the same thing, to a chiefly member of it.

When the motive was acquiring territory, the likely systemic relations were, at least in a gross way, fairly obvious ones. As the pressure of the people upon their existing territory would increase, so would their motivation to fight in order to conquer additional territory, and the growing size of a group would enhance not only the territorial ambitions of its members but also the likelihood of its success in both conquering and exploiting another group's land. It has not been usual in discussions of Maori warfare to regard this territorial motive and the revenge motive as operating within a single system. Indeed, one scholar has gone so far as to suggest that each motive was characteristic of a different phase of Maori prehistory.[7] This, however, is an interpretation inconsistent with the available evidence,[8] and it is possible to set forth an alternative interpretation showing systemic connections between the motives and between the fighting they lead to.

It must be noted first that Maoris committing offenses that had to be avenged could have often been expressing in this way, consciously or unconsciously, the need of their groups for more territory. This is perhaps most obviously so in the case of offenses such

[7] Peter H. Buck (Te Rangi Hiroa), *The Coming of the Maori* (Wellington, 1949), 381, 387.

[8] Thus, the conquest of the Auckland isthmus (see n. 20) occurred during what Buck regards as a period of stabilized land when wars were not fought for territory.

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as trespass or poaching on other groups' food preserves, but murders and other physical assaults and even insults could also result from the increasing pressure of population upon the land. A concomitant of this pressure would be a diminishing per capita food supply and increasing intra-group competition for resources, both of which would contribute to domestic frustrations and other in-group tensions which might eventually find release in verbal or physical violence against members of other groups. Possible illustrations of this process are the examples that some authors give of a "peculiar custom" or "curious idea" of the Maoris: the murder of outsiders by someone because he has been injured or aggrieved by members of his own group and because he wishes therefore to "put his own people in the wrong" and thus make them subject to reprisals from the murder victims' groups.[9] However, the occurrences of this practice were no doubt rare, and many offenses may have served to give vent to hostile or aggressive feelings without any deliberate intention of incurring the wrath of another group.

The offended groups did not have to retaliate immediately.[10] They could bide their time, and, indeed, the memory of unavenged injuries was sometimes handed down from generation to generation like an heirloom. The Maoris had, moreover, some non-warlike means for assuaging the desire for revenge. Thus, a more or less virulent song could be composed and sung. Or witchcraft could be performed. Some groups, knowing themselves to be weak, undoubtedly tried to remain at home in peace rather than march against stronger groups that had insulted or injured them. But this policy would, in effect, be an invitation to the offending groups to continue their offenses and to increase their frequency and magnitude.[11] Eventually a weak group might then be confronted on its home grounds with a powerful attacking force, consciously

[9] Thomas Wayth Gudgeon, *The History and Doings of the Maoris* (Auckland, 1885), 28-29; Edward Shortland, *Traditions and Superstitions of the New Zealanders* (2d ed.; London, 1856), 20; Edward Tregear, *The Maori Race* (Wanganui, 1904), 326-27.

[10] The first part of this paragraph is based on Vayda, *Maori Warfare*, 43-45.

[11] This is well expressed by a chief in one New Zealand writer's account of a Maori council of war: "To pass over such an unprovoked insult would be to end in the truth of the accusation of cowardice. . . . If we do not take revenge for this act of aggression they will become bolder, and it will be unsafe for us to move"; John White, *Te Rou; or the Maori at Home* (London, 1874), 16-17.

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motivated now to annihilate or rout the group and to take over its territory. Yet the policy of non-retaliation was not totally futile, for it could also happen that the enemy would have its hands full with campaigns against other groups and it might even encounter some reverses rendering it incapable of further attacks.

In any event, Maoris were strongly motivated to pursue policies of retaliation rather than non-retaliation. Suffering injuries meant to them a spiritual as well as physical weakening and, accordingly, required the injured to strive to heal themselves by deeds that would restore their repute, their courage, their feeling of strength and wholeness. This way of thinking about injuries and about the necessity for avenging them are said to have been among the first lessons taught to Maori children. By the time the children were grown, they regarded revenge to be a vital point of honor and one of the most important duties of man—something to be pursued in spite of danger and difficulty and every fatigue and privation.[12]

It seems likely then that unless a group was so weak that its members could not possibly entertain some hope of success in a warlike enterprise, revenge would be sought by force of arms against offending groups. And the greater its sense of offense, the greater the incitement to its warriors. The offenses would be recounted with vehemence at public meetings which also featured inflammatory, warlike songs and chants and displays with weapons. The men would become eager to gird themselves with their flaxen war belts and set forth as a *taua toto*, a war party with the object of blood revenge for injuries received.[13]

[12] Revenge as a Maori value has often been discussed. See, for example, Elsdon Best, *The Maori* (Wellington, 1924), II, 232; Buck, *Coming of the Maori*, 388; James Busby, *Authentic Information Relative to New South Wales and New Zealand* (London, 1832), 62; Hugh Carleton, *The Life of Henry Williams, Archdeacon of Waimate, (ed.)* J. Elliott (Wellington, 1948), 178; James Cook, *The Journals of Captain James Cook on His Voyages of Discovery*, (ed.) J. C. Beaglehole (Cambridge, 1967), III, 71; W. E. Gudgeon, "The Toa Taua or Warrior," *Journal of the Polynesian Society*, 13 (1904) 258-60; J. Prytz Johansen, *The Maori and His Religion in Its Non-Ritualistic Aspects* (Copenhagen, 1954), 61-83; Shortland, 230; S. Percy Smith, *The Peopling of the North* (Supplement to the *Journal of the Polynesian Society*, 1896-97), 72; and S. Percy Smith, *Maori Wars of the Nineteenth Century* (Christchurch, 1910), 329-30.

[13] For descriptions of Maori councils of war, see Buck, *Coming of the Maori*,

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Blood revenge was satisfied by the initial shedding of the enemy's blood. Indeed, according to some writers on Maori warfare, the procedure for avenging a murder was simply to proceed in secret to the offenders' place and there slay the first person encountered. By killing any member of the offenders' group, the warriors were revenged and could then return home with the satisfaction of a mission accomplished, even if circumstances precluded their inflicting further bloodshed on their enemy.[14] When a Maori war party had the opportunity, however, it went on killing after its initial shedding of blood.[15] If, for example, the enemy was completely surprised by the attack and offered no initial resistance, the number of lives taken could be so great as to make it the enemy's turn to feel that it had to obtain vengeance by force of arms. This must have happened often, since a major part of Maori tribal histories consists of what one Maori scholar calls a "seesaw record of military exploits."[16]

The existence of this pattern of raids and counter-raids for revenge is what may have led some scholars to warn against thinking of Maori warfare in the same terms as warfare among so-called civilized peoples and even to suggest that "its psychology was more that of a seriously taken game" and that it might "almost be termed a manly physical exercise."[17] Certainly the small scale and short duration of active hostilities and the prominence of the revenge motive set Maori warfare apart from the type of warfare carried on by armies of state-organized societies. However, the contrasts must not be allowed to obscure important similarities, such as, for example, the fact that both types of warfare can result in territorial conquests and the redistribution of population. This

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389; John Savage, *Savage's Account of New Zealand in 1805*, (ed.) A. D. McKinlay (Wellington, 1939), 35, 37; White, *Te Rou*, chap. 2.

[14] Arthur S. Thomson, *The Story of New Zealand* (London, 1859), I, 124; Buck, *Coming of the Maori*, 388.

[15] On this, see the section on "Pursuit and Mortality" in Vayda, *Maori Warfare*, 83-92.

[16] Buck, *Coming of the Maori*, 388.

[17] Ernest Beaglehole, "The Polynesian Maori" in I. L. G. Sutherland (ed.), *The Maori People Today* (Wellington, 1940), 63; Buck, "The Passing of the Maori," *Transactions and Proceedings of the New Zealand Institute*, 55 (1924), 364; cf. Best, II, 225.

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admonition needs to be underscored, because just this kind of obscuring of similarities has taken place in the writings of numerous social scientists on the subject of primitive and modern, or state-organized, warfare.[18] We are speaking here about social scientists who have based their generalizations upon their reading of ethnography rather than upon their own field work, and it is possible that what has misled them into seeing only blood feuds and revenge and not territorial conquests in primitive warfare is something referred to earlier: the general failure of ethnographers to note the systemic relations between the fighting undertaken for revenge and the fighting undertaken for territory. The crucial observation which the ethnographers have not made is that fighting for revenge, along with the offenses that give rise to it, can be part of a process of recurrent testing of manpower—the manpower available both for defending and for exploiting the land. In this process, groups passed the first part of their tests by attacking when strongly provoked and by defending themselves stoutly when attacked. The warfare in which they were involved at this stage can appear to have no relation to the conquest and use of land and to be only a series of raids and counter-raids for revenge or glory or exercise, but it is necessary to take due notice of more of the testing process. Committing offenses and fighting for revenge formed only the first part.

Following the process simply to the point where some groups failed to defend themselves or to retaliate adequately and their enemies, perceiving this, responded with conscious attempts to conquer their land is itself sufficient to indicate a connection between fighting for revenge and fighting for territory. The testing process did not end, however, with outright attempts at taking land. If an attempt was successful to the extent that a group, having had a succession of victories over one section after another of a particular enemy tribe, was able to move into the territory of its beaten foe, some of the latter might still have

[18] See, for example, Alexander Lesser, "War and the State," in Fried, 95; Julian H. Steward and Demitri B. Shimkin, "Some Mechanisms of Sociocultural Evolution" in Hudson Hoagland and Ralph W. Burhoe (eds.), *Evolution and Man's Progress* (New York, 1962), 79; Quincy Wright, *A Study of War* (2d ed.; Chicago, 1965), 73-74.

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tried to maintain a claim to the territory by "keeping their fires alight" somewhere within it, i.e., by keeping their cooking-fires going and by digging fernroot, farming small pieces of ground, snaring birds, trapping rats, and pursuing other economic activities in some mountain or forest places not readily accessible to the conquerors. Such methods were used by defeated people as a stopgap, something to be continued until they could reconstitute their forces and try to repossess their entire territory. Conquerors could achieve no recognized rights to taken lands without actually occupying them to the utter exclusion of the original owners,[19] and a long time could elapse before the victors could be certain of having accomplished this. Defeated people sometimes spent years in refuge at the settlements of friends and relatives and then came to try to wrest their land back from the conquerors.[20] In other words, the testing process had safeguards reducing the likelihood that groups with good fortune in war would take over land which they could exploit no better than could the vanquished. Military triumphs resulting from clever strategies or from luck could not lead to successful conquests if the victorious groups did not have the numbers and strength to use the defeated people's land and to resist their efforts to recover it.

Some warnings may be in order here. No extraordinary efficiency should be imputed to the process that we have been elucidating. For one thing, it could impair for both victors and vanquished, through the toll that it exacted in resources and lives, the very thing that it was testing for: the capacity to defend and exploit the land. However, our purpose has been simply to show that there was a testing process among the Maoris and that it involved fighting for revenge as well as fighting for territory. Noth-

[19] On this and related aspects of Maori land tenure, see F. O. V. Acheson, "Maori Land Customs and Education: Their Inter-Relation" in P. M. Jackson (ed.), *Maori and Education* (Wellington, 1931), 133-43; Raymond Firth, *Primitive Economics of the New Zealand Maori* (London, 1929), chap. 11; Norman Smith, *Native Custom and Law Affecting Native Land* (Wellington, 1942); J. White, "Maori Customs and Superstitions" in T. W. Gudgeon, 183-216.

[20] For an early nineteenth century example, see Leslie G. Kelly, *Tainui* (Wellington, 1949), 297-98. How drawn out the testing process could be is well illustrated also by the mid-eighteenth century conquest of the Auckland isthmus from the Wai o Hua people by the Ngati Whatua; see the accounts in Smith, *Peopling of the North*, 83-91, and Kelly, *Tainui*, 253-57.

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ing that has been said would warrant the conclusion that some alternative, which, for example, would have involved less bloodshed and killing and would not have required men to be always apprehensive and in dread that their enemies would surprise them, would not have worked better for the Maoris. The process that evolved is the object of our analysis because it was the process operating among them and not because it was necessarily the best way of doing things.

It must be emphasized that the Maoris did not see the process as a means for testing the manpower of groups and for correcting disparities between them in their man/resource ratios. Evolutionary development often results in processes with systemic properties not manifest to the people involved.[21] Characteristically, the long-term adaptive advantages that accrue to populations as a result of such processes are, in some way, not sufficient rewards to individuals—for example, not sufficiently obvious, immediate, palpable—to be effective spurs to the actions required to keep the processes going. Accordingly, these actions are undertaken for other rewards which are more or less immediate, not necessarily material, and often taught to be morally desirable. Revenge in the Maori case is a good example of such rewards. People would act for revenge, even if they were oblivious that their actions might help provide exploitable environments to posterity.

We may say then, in summary, that in pre-European times the Maoris were motivated to commit offenses, take revenge, conquer land, and engage in various other actions that constituted, without their necessarily knowing it, essential operations of a system whereby the overall population of New Zealand could grow and be dispersed and whereby access to the resources sustaining this

[21] Some other examples of this are cited in Andrew P. Vayda and Roy A. Rappaport, "Ecology, Cultural and Noncultural" in James A. Clifton (ed.), *Introduction to Cultural Anthropology* (Boston, 1968), 491-92. The "blind" variation and selective trait-retention that result in processes of the kind referred to are emphasized in some general discussions of sociocultural evolution; see, for example, Donald T. Campbell, "Variation and Selective Retention in Socio-Cultural Evolution" in Herbert R. Barringer *et al.* (eds.), *Social Change in Developing Areas* (Cambridge, Mass., 1965), 26ff.; George Peter Murdock, "How Culture Changes" in Harry L. Shapiro (ed.), *Man, Culture, and Society* (New York, 1956), 252ff.; Vayda, review of *The Evolution of Culture* by Leslie A. White, *Canadian Journal of Economics and Political Science*, 26(1960), 646-47.

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process could be regulated. We shall see that European influence was disruptive to this adaptive system and resulted in heavy mortality, not because there was any waning of the traditional motivations but because many of the actions based on them had new effects after the introduction of a new technology of warfare and were no longer geared to local population pressures and to variations from group to group in the manpower for defending land and exploiting the available resources.

New Weapons in the Service of Old Objectives

We must speak first about the Maoris' new technology of warfare, and this means speaking, above all, about guns. These were not the only new weapons, for European hatchets, axes, and knives also came to be used in battle, but guns were the most significant introduction. The Maoris began to acquire them in the early years of the nineteenth century, and the first encounters with the weapons filled the people, understandably enough, with fear and awe.[22] It did not take long, however, for some Maoris to acquire a more practical understanding of guns and their uses. This happened first along the eastern coasts of the northern part of New Zealand's North Island, an area which, initially, received the most frequent calls from trading and whaling ships. By 1814, when members of the Church Missionary Society came to the Bay of Islands in that area to establish a station, a local chief, Hongi Hika of the Nga Puhi tribe, had already been able to teach himself to stock and mount a musket without any help and he owned several of the weapons.[23] As we shall see, Hongi's affinity for guns eventually made him and his people a scourge to other tribes.

The first guns obtained by the Maoris from the Europeans were flintlocks, often of poor quality, since European traders had no great scruples about taking advantage of the Maoris' ignorance of firearms. Yet, as soon as guns came into use in Maori warfare,

[22] See the accounts in John Liddiard Nicholas, *Narrative of a Voyage to New Zealand* (London, 1817), I, 253-54; J. White, *The Ancient History of the Maori* (Wellington, 1887-90), V, 172; Best, II, 286.

[23] Harrison M. Wright, *New Zealand, 1769-1840* (Cambridge, Mass., 1959), 86.

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they were effective enough. Initially, one or two guns sufficed to give any war party the advantage in fighting against enemies unacquainted with the weapons, not so much because of the numbers killed by the guns as because of the inevitable panic. A good illustration of this is the success of a party of northern warriors who, in 1818, were the first to use guns against the Taranaki peoples of the central western coast of the North Island. The attackers had only two old flintlocks, which they employed to shoot the chiefs of each enemy *pa*, or fortified village. When the defenders heard the noise of the guns and saw their leading warriors fall without, as far as they could see, having been struck, they concluded that supernatural forces were at work, that gods had joined in the fighting. Accordingly, when the attackers stormed a *pa*, they found their enemy panic-stricken and offering no effective resistance to being slaughtered with the traditional Maori clubs and spears. After every battle, the northern war party remained to feast on the flesh of the slain, taking care to leave no survivors to carry the alarm to the next settlement.[24]

During this early period, the Maoris calculated the strength of war parties not so much by the number of men they had but by the number of muskets. In 1819, a party with twelve muskets was considered strong, while one armed with fifty guns was regarded as having terrifying power. As increasing numbers of Maoris learned about the effectiveness of the new weapons, an arms race developed. By 1820, English visitors to the Bay of Islands were estimating that the Maoris there had "some hundred stand of arms" or even as many as 500, and in the following year a Bay of Islands missionary wrote that he did not think there could be "less than two thousand stands of Arms among the Natives."[25] By 1826, one of the missionaries was speaking

[24] Discussions similar to mine in this paragraph may be found in H. Wright, 84-85, and W. T. L. Travers, "The Life and Times of Te Rauparaha," *Transactions and Proceedings of the New Zealand Institute*, 5 (1872), 46-47. My account of the expedition against the Taranaki peoples is based on S. Percy Smith, *History and Traditions of the Maoris of the West Coast* (New Plymouth, 1910), 286; and Archdeacon P. Walsh, "The Passing of the Maori," *Transactions and Proceedings of the New Zealand Institute*, 40 (1907), 157.

[25] Cited in H. Wright, 91. Chapter 5 of Wright's book is a valuable secondary source on the introduction and spread of muskets among the Maoris. Other sources that I have used in writing this and the next six paragraphs on the

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about "many thousand stands of arms." Tribes at some distance from the favored ports of call for European ships were less well supplied, but once they had suffered from the power of guns, they made the most strenuous efforts they could to obtain them. The growing overseas demand for such New Zealand products as flax and timber ensured a flow of muskets and powder from the Europeans' trading vessels to the Maoris, for the latter increasingly would accept nothing else in exchange for the goods which the foreigners sought. Even the missionaries were sometimes reduced to trading guns to the Maoris in order to obtain food supplies or timber from them. In time, the whole North Island became something like one great camp of musket-armed warriors.

The destructive use of the new means of war in pursuit of the traditional goal of revenge is epitomized in the post-European career of Chief Hongi Hika. In 1820, desirous of obtaining arms, he arranged to be taken to England by the missionary, Thomas Kendall. Hongi was presented to King George IV and met a number of other influential persons, who became impressed with his intelligence and his professions of desire to work for the welfare of his people. These professions were not necessarily hypocritical, since, to Hongi, his people's welfare depended upon their aggressively avenging past defeats and injuries. Although he was unsuccessful in obtaining more than two or three guns in England, he did receive numerous gifts of plows and other agricultural tools to help him bring civilization to the Maoris. On the return voyage, Hongi stopped in Sydney and there traded his stock of

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use of muskets in Maori warfare are Judith Binney, *The Legacy of Guilt* (Christchurch, 1968), 85 ff.; T. W. Gudgeon, chaps. 11, 13-21; Francis Hall, "Extracts from the Journal of Mr. Francis Hall" in *Missionary Register for MDCCCXXIII* (London, 1823), 504-08; James Hamlin, "On the Mythology of the New Zealanders," *Tasmanian Journal*, 1 (1842), 345-54; René P. Lesson, *Voyage autour du monde* (Paris, 1839), II, chap. 21; Smith, *Maori Wars*, 15-18 and *passim*; E. J. Tapp, *Early New Zealand* (Melbourne, 1958), chap. 3 and p. 178; Thomson, I, 258-62, 298-99; Travers, 47; Walsh, 157-58; and White, *Ancient History*, V, 168-76. My account of the offenses that Hongi Hika of the Nga Puhi was avenging in his expeditions is based mainly on Smith, *Maori Wars*, 46-49, 177-79, 182-84, 198, 204-07, 225, 241-42 and 330; cf. L. G. Kelly, "Fragments of Ngapuhi History," *Journal of the Polynesian Society*, 47 (1938), 179-80; Samuel Marsden, *The Letters and Journals of Samuel Marsden 1765-1838*, (ed.) J. R. Elder (Dunedin, 1932), 242-43.

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implements and other gifts for muskets and powder. These transactions provided him and his people with an armory of 300 guns and a new supply of ammunition.

Less than two months after returning to the Bay of Islands in 1821, Hongi was ready to put the new arms to use. An expedition for revenge was organized against two tribes living to the south, the Ngati Paoa and Ngati Maru of the Hauraki Gulf and Firth of Thames area. A member of the Ngati Paoa tribe, probably in the previous year, had treacherously killed a Nga Puhi man and the Ngati Maru had defeated Hongi's Nga Puhi in a battle fought with native weapons in the last decade of the eighteenth century. Such murders and defeats called for revenge, and Hongi now had the means of obtaining it. The expedition was of unprecedented magnitude—one of the missionaries, Francis Hall, estimated that the expedition was going to have "at least 1000 muskets and perhaps more than double that Number of Men"—and it was successful. The relatively few muskets of the people attacked were insufficient to make them any match for the northern forces, and the ramparts and other traditional defenses of the *pa* of Hongi's enemies afforded them little protection when Hongi's men could erect wooden platforms to serve as elevated vantage points for the firing of musket balls. In their attacks on only one Ngati Maru *pa* and two Ngati Paoa ones, the Nga Puhi killed some two thousand people and, according to some accounts, took an equal number as prisoners.

Soon after this, Hongi rallied his followers for another and even more formidable expedition. According to one missionary observer, there were over three thousand warriors in the force that embarked from the Bay of Islands at the end of February 1822. The victims this time were to be the people of the Waikato River against whom the Nga Puhi had a number of *take*, or causes for war. There was an unavenged Nga Puhi defeat in battle as well as some specific deaths for which the Waikato were held responsible, especially the deaths of two young chiefs who had been killed in the previous year as they were attacking the Ngati Maru *pa*, which some Waikato were helping to defend. On hearing about the new Nga Puhi expedition, the Waikato people, who had no guns, assembled in a single, very large *pa*, perhaps in the hope that their numbers alone (variously estimated at between 5,000

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and 10,000 people) might save them. This was not to be the case. Indeed, the issue was decided almost immediately after Hongi's warriors arrived at the *pa* and disembarked from their canoes. The guns were fired, and the people inside the *pa*, many of whom had not previously seen the European weapons in operation, began to flee in dread. As the firing increased, so did the panic of those inside. Many of the fleeing fell into the ditch surrounding the *pa*, and the first to fall had others fall on top of them with the result that hundreds were smothered or trodden to death. The Waikato warriors who waited to meet the attackers in close combat were shot as they rushed up to be near enough to the enemy fighters to use their wooden weapons. The Waikato were driven from the *pa* and, by the estimate which Hongi subsequently gave to the missionaries, lost 1500 people. Nga Puhi deaths in the engagement were but a small fraction of this.

Before Hongi's death in 1828, he and other Nga Puhi chiefs led campaigns against tribes of various other districts, including Kaipara, which is near the Bay of Islands, and the Bay of Plenty and the East Coast, which are far away, farther than any Nga Puhi war parties had ranged prior to the nineteenth century.[26] A main objective in these campaigns, which resulted in the slaughter of several thousand more Maoris, was still vengeance. Thus, the major Nga Puhi expedition in 1823, against a Bay of Plenty tribe called Te Arawa, was in revenge for a Te Arawa subtribe's treacherous massacre of a Nga Puhi party in 1822. And Hongi's last great victory, against the Ngati Whatua of Kaipara in 1825, was the Nga Puhi's revenge for a series of stinging defeats in pre-musket times, particularly a defeat in which 150 or more Nga Puhi had died at Moremo-nui stream in 1807. Hongi's father and half-brother were among those who had fallen in battle at Moremo-nui, and his sister had also been killed there by some Ngati Whatua, who, before Hongi's eyes, had first subjected her body to various abuses and had then thrown her alive onto the hot oven stones. It is said that Hongi's main purpose in making his trip to England had been to secure arms for avenging the deaths at Moremo-nui.

[26] According to the traditional accounts, the Hauraki Gulf was the furthest south that any expedition of northern warriors penetrated until the early years of the nineteenth century; Smith, *Maori Wars*, 17. Cf. Firth, 430.

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Their expeditions earned Hongi and the Nga Puhi an early notoriety, but there were others, like the Ngati Toa led by Te Rauparaha and the Ngati Haua led by Te Waharoa, who also raided widely, frequently, and fiercely once they got hold of muskets. Te Rauparaha and his men even carried the new brand of warfare across Cook Strait to the east coast of the South Island where they raided as far south as Kaiapoi near the present city of Christchurch. In the North Island, there was hardly any district spared from the depredations of warriors armed with guns.

These other aggressors of the musket era were also, for the most part, bent upon revenge. Acquisition of guns enabled them to square accounts with numerous traditional enemies at whose hands they had suffered defeats earlier. And, it should be noted, traditional enemies were not the only ones against whom guns were directed by revenge-seekers in the 1820s and 1830s. Any injuries received from friends and allies of an attacked foe had also to be avenged and now for the first time, with guns available, they readily could be. This is illustrated by Hongi's expedition against the Waikato. Te Rauparaha's first attack upon the Ngai Tahu of the South Island was similarly motivated by a desire for revenge, for some Ngai Tahu had participated in an unsuccessful attack upon Te Rauparaha's forces on Kapiti Island near the present city of Wellington and one of their chiefs was known to have subsequently uttered the insulting boast that "if ever Te Rauparaha dared to set foot on his land, he would rip his belly open with a shark's tooth."[27]

In the case of Te Rauparaha's wars, the traditional motive of acquiring land was also operating. Some writers have interpreted territorial conquests by Te Rauparaha and other Maoris in the musket era as evidence of certain new motivations for war—motivations which, for convenience, we might call "Napoleonic," since they are alleged to have been patterned on the example of Napoleon's career of empire-building in Europe.[28] The interpretation cannot be said to be well founded, for, regardless of the knowledge that

[27] T. Lindsay Buick, *An Old New Zealander or, Te Rauparaha, the Napoleon of the South* (London, 1911), 122.

[28] For examples of this interpretation, see Smith, *Maori Wars*, 15 and the citations in H. Wright, 119-21.

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particular chiefs may have had of Napoleon's career, it remains that the conquests of the 1820s and 1830s were cast much more in the traditional Maori mold than in any Napoleonic one. No evidence exists that effective political consolidation was a result or even an aim of the conquests. Defeated people were not, as a rule, kept on their former land and made politically subject to their conquerors. Instead, following the traditional pattern, conquerors, for the most part, killed members of the defeated group and excluded them from their former territories. Te Rauparaha, designated by one biographer as the "Napoleon of the South," does not seem to have been an exception to this. He is described by the same biographer as having preached to his followers a doctrine of exterminating the North Island tribes whose lands in the Wellington vicinity the Ngati Toa were taking.[29]

In the campaigns led by Hongi, there were never even any attempts to take over the lands of defeated enemies. And this was the chief whom the missionary, Samuel Marsden, had proposed as the person to become a king and bring peace and stable government to New Zealand.[30] The fact is that Hongi was never fully successful in securing his authority even within his own tribe. As long as he was well supplied with guns and other Nga Puhi chiefs were not, he could call out all the sections of the tribe for his expeditions, for none wished to have Hongi's guns directed against themselves. Even so, in the attack on one Ngati Paoa *pa* in the expedition of 1821, the men of four or five of the Nga Puhi subtribes asserted their independence by refusing to take part, although they did join their tribesmen in the remainder of the campaign, and the bullet wound from which Hongi eventually died was received in a battle in which some of his former followers were arrayed against him. Later, when guns were general among the Maoris, according to one missionary the war cry could go from village to village in the Bay of Islands without any effect, and only with the "utmost difficulty" could a sufficient number of men be mobilized to take part in fighting beyond their own

[29] Buick, 87. Buick's book suggests (p. 126) that Te Rauparaha's policy in the South Island may have been different, but the details are hazy.

[30] Marsden, 386, 388.

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immediate districts.[31] There was no fuel here for Napoleonic ambitions. The reasonable interpretation is the one suggested earlier: that new means were being used in the service of traditional objectives.

The Maladaptive Effects

The traditional system was disrupted with the weakening of the nexus between Maori warfare and variations among groups in population pressure and in manpower, and this weakening was effected partly through disruption of the localization of the system. The revenge-seekers of the musket era were turning their guns not only upon their nearby enemies with whom the traditional testing process could operate in pre-European times through repeated offenses, raids, and counter-raids and regulated escalations to attempted territorial conquests. It must be remembered that the Maoris themselves understood revenge rather than any testing process to be the object of their expeditions, now undertaken to distant places in retaliation for offenses, which could not even have been committed if not for Maori intercourse with Europeans. For example, when European ships landed Maoris in parts of New Zealand far from their homes, they put ashore stowaways, temporary seamen who could not be taken back to England, men intended for labor in timbercutting, and even women sold as slaves. Conflicts inevitably developed between some of these Maoris and the local inhabitants, resulting in insults and injuries which constituted intertribal offenses involving tribes that previously had had virtually no intercourse with one another. Such problems developed when a group of the women, abducted in 1806 from various points in the northern part of the North Island by a crew of European mutineers on the brig *Venus*, were eventually sold by the sailors to chiefs in the Bay of Plenty and East Coast districts. There they served for a time as wives and slaves, but when quarrels arose, were put to death. It was in revenge for these of-

[31] William Yate, *An Account of New Zealand* (London, 1835), 119. My other sources on the question of Hongi's authority are Marsden, 388; Smith, *Maori Wars*, 189, 398; Vayda, *Maori Warfare*, 33; White, "Maori Customs and Superstitions," 222-23; and H. Wright, 120-21.

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fenses that some of the earliest Nga Puhi expeditions against distant tribes took place.[32]

Other expeditions beyond the pre-European range of military operations resulted from the fact that any injuries received by an attacking force from friends and allies of the enemy could now, with guns available, also be readily avenged. Hongi's expedition against the Waikato people in 1822 illustrates this, since he was, as previously noted, avenging Nga Puhi deaths inflicted by Waikato warriors in the course of their defense of a Ngati Maru *pa* in 1821. Te Rauparaha's first expedition across Cook Strait against the Ngai Tahu of the South Island was, it should be recalled, similarly motivated.

Such expeditions against faraway tribes could not be undertaken before the nineteenth century. When there were no guns to give security to attacking parties, distant places were full of peril for them, because any people attacked there would have had many friends and allies nearby. The introduction of guns disrupted also whatever correspondence had previously obtained between success in warfare and the relative manpower of groups for exploiting land. Maori local groups in the early part of the musket era were being confronted with attacking forces drawn not from one or two enemy villages or subtribes but rather from many villages and even from different tribes. Such forces were veritable armies and could be formed because the prospect of victory by virtue of having guns drew warriors together from different groups with common foes.[33] The ease with which such forces defeated enemy village communities one by one is illustrated by the success of the northern tribes' expedition along the west coast of the North Island in 1819 and 1820, an expedition that took warriors of the

[32] My discussion of Maoris put ashore away from their homes follows H. Wright, 83-84. On the abductions by the *Venus* crew, see also Smith, *Maori Wars*, 56-57, 90, 156, and the references cited in C. W. Vennell, *The Brown Frontier* (Wellington, 1967), 179.

[33] It has, in fact, been suggested that the people called Nga Puhi in the 1820s included some Bay of Islands and Hokianga groups not previously regarded as belonging to the Nga Puhi tribe. This greater Nga Puhi complex may have been "an unstable political association brought about by the advent of the musket to the Bay"; Binney, *Legacy of Guilt*, 58, n. 6. Cf. Jean Kennedy, *Settlement in the South East Bay of Islands, 1772* (University of Otago, Dunedin, 1969), 88 (Mimeo).

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Nga Puhi and Te Roroa tribes as far south as Wellington and involved the capture of one *pa* after another en route and the slaughter of those attacked and the consumption of their flesh.[34] Clearly there was no test of the relative capacity of tribes to use the land when engagements were between the members of a single local group or village community of one tribe and an attacking force composed of the men of the various sections of two different tribes. Also, the attackers obviously were greatly aided by having guns. Even gathered in one place and having the advantage of numerical superiority, a tribe armed with only native weapons was no match for musket-armed attacking forces. Instead of reflecting the relative capacities of groups to use particular territories, outcomes of engagements in the early part of the musket era reflected mainly the relative success of groups in obtaining guns.

Moreover, in addition to failing to obtain the benefits of the traditional system, the Maoris in the musket era suffered other, ultimately lethal adversities because of the use of new means of warfare in pursuit of traditional goals. There was, first of all, a substantial increase in mortality resulting directly from warfare. The Nga Puhi were said to have killed some 3,500 people in only their first two expeditions after Hongi's return from England. This estimate may be exaggerated to some extent, but it is probably safe to assume that the people slaughtered in the course of Hongi's several expeditions in the musket era did number in the thousands. It is true that in the later fights when both sides were well armed with guns, the number of casualties relative to the number of combatants may often have been less than in the old days of hand-to-hand fighting when many men could be killed once a rout had commenced.[35] Moreover, warfare began to decline in the

[34] Described in Smith, *Maori Wars*, 96ff.

[35] Proceedings in the case of routs in pre-musket times are described in F. E. Maning, *Old New Zealand, a Tale of the Good Old Times; and a History of the War in the North* (London, 1876), 147-48, by an old Maori who had been active in fighting from the 1770s on:

> . . . when once the enemy broke and commenced to run, the combatants being so close together, a fast runner would knock a dozen on the head in a short time; and the great aim of these fast-running warriors . . . was to chase straight on and never stop, only striking one blow at one man, so as to cripple him, so that those behind should be sure to overtake and finish him. It was not uncommon for one man, strong and swift of foot, when the

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1830s as the various tribes and subtribes succeeded in equalizing their supplies of firearms. However, the great increase in the scale and frequency of fighting in the first part of the musket era no doubt did result in a much greater total number of deaths in Maori warfare than had ever occurred previously in a comparable period of time.[36]

To the mortality from battle itself must be added the deaths caused in other ways by the guns.[37] Starvation resulted from the new warfare and from the race to acquire the arms for engaging in it. Despoilation of gardens and other food preserves by enemy expeditions contributed to food shortages, while continual attacks or the fear of them made it impossible for the people of some districts to give due attention to the production of food. There was, moreover, widespread neglect of subsistence labors in general as all Maoris applied themselves to the tasks of producing the flax and other goods that could be traded to Europeans for guns and ammunition.[38] One European observer, F. E. Maning, who lived with the Maoris from 1833 on, noted that for one or two muskets the people had to give a ton of flax, "scraped by hand with a shell, bit by bit, morsel by morsel, half-quarter of an ounce at a time." Maning's conclusion was:

> Now as the natives, when undisturbed and labouring regularly at their cultivations, were never far removed from necessity or scarcity of

enemy were fairly routed, to stab with a light spear ten or a dozen men in such a way as to ensure their being overtaken and killed.

[36] On battle mortality in the musket era, see Smith, *Maori Wars*, 17-18 and *passim*; J. Rutherford, ms, cited in Judith Binney, "Christianity and the Maoris to 1840: A Comment," *New Zealand Journal of History*, 3 (1969), 149, n. 30; and the sources cited in Vayda, *Maori Warfare*, 86.

[37] Sources on this include Gordon Lewthwaite, "The Population of Aotearoa: Its Number and Distribution," *New Zealand Geographer*, 6 (1950), 41-42; Maning, 158-63; Edward Markham, *New Zealand or Recollections of It*, (ed.) E. H. McCormick (Wellington, 1963), 46-47; Marsden, 386; Polack, II, 344; Vayda, *Maori Warfare*, 69; Walsh, 158-59; and H. Wright, 76-78.

[38] Production for trade rather than for Maori subsistence is also the work to which were assigned the large numbers of slaves being kept for the first time by some tribes. In earlier days, each group had kept only a few prisoners of war as slaves and had used them only in menial tasks connected with cooking and burden-bearing. I have noted elsewhere how the guns changed this; see my *Maori Warfare*, 106-07, and "Maori Prisoners and Slaves in the Nineteenth Century," *Ethnohistory*, 8(1961), 144-55.

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> food, we may easily imagine the distress and hardship caused by this enormous imposition of extra labour. They were obliged to neglect their crops in a very serious degree, and for many months in the year were in a half-starving condition, working hard all the time in the flax swamps.[39]

As Maning's statement suggests, the Maoris, because of the guns, were not only neglecting their subsistence labors but were also working themselves to exhaustion to produce goods for trade. The guns had yet another effect which made the Maoris easy prey to pulmonary disease and death: the move from airy settlements on high ground to makeshift residences by the swamps where the flax grew best. Hilltops, to which food, fuel, and water had to be carried from below, had once provided advantages for defense against attacking parties, but they did no longer. Time, made newly precious by having to work in order to buy guns, could be saved by the Maoris living near the flax and timber and also near the available sources of provisions, and the people descended to the lowlands where,

> in mere swamps they built their oven-like houses, where the water even in summer sprung with the pressure of the foot, and where in winter the houses were often completely flooded. There, lying on the spongy soil, on beds of rushes which rotted under them—in little, low dens of houses, or kennels, heated like ovens at night and dripping with damp in the day . . . and impossible to ventilate—they were cut off by disease in a manner absolutely frightful.[40]

Accurate figures on the number of Maoris that perished in one way or another because of warfare in the early nineteenth century are unavailable, but nobody seems to have estimated the number at less than 20,000 and some who have reckoned it as high as 80,000 have suggested that between one-quarter and one-half of the total number of Maoris in New Zealand was lost.[41] Even if we allow for substantial exaggeration in the last estimates the Maori losses on account of the guns were considerable, and these losses, it must be recalled, were without the kinds of offsetting gains that

[39] Maning, 162-63.
[40] *Ibid.*, 160.
[41] Various estimates are cited in Lewthwaite, 42-43. See also Thomson, I, 261, and H. Wright, 102.

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had been formerly accruing from the operation of the traditional system.

If we can say then that the warfare of the musket era was maladaptive for the Maori population as a whole, it becomes noteworthy that the course of developments which we have been considering is consistent with the concept of "cultural lag" as used to describe the genesis of maladaptation among human populations. That is to say, here, as in many other cases, change in certain practices that had been adaptive lagged behind other changes which were making the practices maladaptive.[42] Fighting for revenge as part of an adaptive system had also become an unquestioned part of what the Maoris regarded as right and necessary and were teaching their children to regard in the same way. Fighting for revenge had become this to such an extent that the Maoris continued to fight for revenge under conditions which not only precluded the operation of the traditional adaptive system but also had other maladaptive consequences. The effectiveness and institutionalization of the earlier adaptation may be said then to have been responsible for the cultural lag resulting in the later maladaptation.[43]

Actually, the lag was very brief in time. By 1830, the value of fighting for revenge was, according to the missionaries' accounts, being questioned by many of the Nga Puhi, who found themselves increasingly beset by death and illness and threatened by their erstwhile victims who now also had guns.[44] Moreover, when

[42] For examples of other cases of this, see Amos H. Hawley, *Human Ecology* (New York, 1950), 27-28, and Vayda, "An Ecological Approach in Cultural Anthropology," *Bucknell Review*, 17 (1969), 1, pp. 112-19. A striking case referred to by the latter is the persistence of infanticide among certain South American Indians whose early contacts with Europeans and their decimating diseases were making underpopulation rather than overpopulation a critical problem. Details of this are given in Charles Wagley, "Cultural Influences on Population: A Comparison of Two Tupí Tribes," *Revista do Museu Paulista*, n.s., 5(1951), 95-104.

[43] An example of generalized interpretive statements along similar lines (although in somewhat different language) is the following by the social psychologist, Donald T. Campbell: "The 'wisdom' produced by biological and societal evolution is retrospective, referring to past environments. It is only adaptive to the extent that these environments remain stable. Yet the rigid preservation systems essential for the process of evolution also provide for a retention of . . . [traditional] systems long beyond their usefulness"; Campbell, 35.

[44] See the citations in H. Wright, 100, 147.

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all the tribes were equipped with muskets, it soon became apparent that easy victories could no longer be won. There was, accordingly, a decline in the resort to arms throughout New Zealand in the latter part of the 1830s. By 1840, the year which saw the beginning of organized European colonization of New Zealand, many Maoris had found in Christianity a new set of values according to which not taking revenge for injuries suffered could, most opportunely, be justified.[45] Firearms were cast aside, to be used again only years later when the Maoris fought to stop the European settlers from taking their lands. And yet the very brevity of the lag in adjusting to the muskets is significant, for we can see in the Maori experience how calamitous and destructive even a brief period can be when potent new technologies are employed by men locked, however temporarily, into traditional, culturally sanctioned patterns of thought and action. This has portents for us in our own time of accelerated technological change, disrupted ecological systems, and distant and costly wars being fought for objectives persisting from the past.

[45] Documentation on the decline in warfare is provided once more by H. Wright (100-01, 147, 159), an invaluable secondary source on almost all aspects of the European impact on the Maoris before 1840. Wright's book includes a chapter on the rapid and massive conversion of the people to Christianity in the late 1830s and early 1840s. See also Binney, "Christianity and the Maoris"; Buck, *Coming of the Maori*, 524; Carleton, 178; and Rev. William Williams, *Christianity among the New Zealanders* (London, 1867), 89 ff., 267-68, and *passim*.

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The Failure of Nazism in America: The German American Bund, 1936-1941

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Today the American Nazi Party is viewed as an insignificant hate group which propagates nonsense and is rent with internal dissension. But in the late 1930s when the German American Bund tried to advance National Socialism in the United States, Americans became alarmed. From the perspective of several decades it is apparent that their apprehensions were exaggerated. Recent studies on Nazi activities in America[1] and an examination of Bund papers[2] reveal that the Bund neither warranted the attention it received nor ever presented a threat to American institutions.

Throughout its brief and stormy history, the German American Bund faced a succession of difficulties and problems. The very nature of its establishment augured badly for the movement. Organized in March 1936 by Fritz Kuhn, a naturalized American

[1] Alton Frye, *Nazi Germany and the American Hemisphere, 1933-1941* (New Haven, 1967); Arthur L. Smith, Jr., *The Deutschtum of Nazi Germany and the United States* (The Hague, 1965); Joachim Remak, "'Friends of the New Germany,' the Bund and German-American Relations," *Journal of Modern History*, XXIX (1957), 38-41; O. John Rogge, *The Official German Report* (New York, 1961).

[2] Record Group 131. APC World War II Seized Records. German American Bund (National Archives, Washington, D.C.). Cited hereafter as R.G. 131.

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citizen born in Germany, the Bund evolved from the Friends of the New Germany, an association which from the time of its formation in 1933 had a bad reputation. The Friends bluntly espoused the National Socialist cause and openly admitted having ties with the Nazi hierarchy in Germany. Such boldness brought sharp reactions in America and Germany. Finally in December 1935, Berlin released a directive ordering all German nationals out of the Friends, which decimated the ranks of the movement and many of its officials returned to Germany. The collapse of the Friends did not dishearten members like Fritz Kuhn. He worked vigorously to salvage the remnants of American Nazism and formed a new organization, the Amerika-Deutscher Volksbund, a large percentage of whose members had belonged to the Friends. The Bund inherited some of the Friends' properties, notably Camp Siegfried on Long Island and the newspaper *Deutscher Weckruf und Beobachter*. Although Kuhn created an affiliated group, the Citizens Protective League, which admitted German nationals into his movement, the chief distinction between the Friends and the Bund was that only American citizens could hold leadership positions in the new organization.[3] Nevertheless, in spite of this change, it required considerable political acumen to build upon an association which had been repudiated in America and Germany, and Kuhn and other Bund leaders soon proved that they lacked such astuteness and moderation.

Between 1936 and 1938 the Bund directed most of its appeals to the German-American community. Pamphlets stating the aims of the organization proclaimed that it existed to unite all German Americans into "one great, nation-wide, respect-commanding movement."[4] This proved to have been an unrealistic goal since the Bund had difficulties enough maintaining unity within its own ranks and could not possibly have united so diverse a population as the entire German-American community. In its

[3] For a more detailed analysis of early American Nazi groups and the Friends of the New Germany see Frye, 32-64; A. L. Smith, 59-90. Other helpful sources are William Francis Hare, *The Brown Network* (New York, 1936) and U. S., Department of Justice, *German American Bund. Outline of Evidence*, Sept. 17, 1942, pp. 1-12.

[4] *Purposes and Aims* (New York, Deutscher Weckruf und Beobachter, n. d.), R.G. 131, Box 1.

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overtures to German Americans, the Bund usually stressed pan-Germanism. Bundists saw a German-American community of thirty million people in a state of flux and still suffering from World War I persecutions. They assumed that by glorifying Hitler and National Socialist Germany, they could exploit these alleged second class citizens. This message was promoted through such propaganda vehicles as the Bund newspaper, Nazi films—notably *The Triumph of the Will*—Hitler's *Mein Kampf*, and numerous Bund pamphlets. Bundists believed that once German Americans recognized Hitler's achievement of uniting the German people under National Socialism they would flock to the Bund inspired and hopeful of duplicating his example in America. For Fritz Kuhn, Hitler's rise to power had reawakened dormant loyalties among Germans everywhere. It proved that all Germans were united in a racial community and bound to the Fatherland by their common blood. With a cohesive nazified German-American community behind him, Kuhn felt German Americans would not only gain a new racial-political significance but be able to exert pressures on the American government to adopt a policy favorable to Germany and to fight Communism more aggressively at home and abroad.[5]

Basic to the Bund's failure to capture a mass following was this attempt to relate a foreign ideology to the American scene. This mindless effort produced ludicrous interpretations of American life and showed that the Bund lived in an eerie world of fantasy and fabrication. Along with the goal of a united National Socialist German America, Bundists' fantasies produced visions of a vast Jewish-Bolshevik conspiracy. They continually issued proclamations stating that Communism was their main target. The Bund's attack on Communism embraced the Nazi concept of *Gesamtmarxismus*, or all Marxist phenomenon, and masked a virulent anti-Semitism. The two themes were linked and the fight against Jewish Bolshevism became the Bund's supreme battle. Waged on the lowest of levels, the struggle often approached a pornographic diatribe.[6]

[5] Fritz Kuhn, *Awake and Act* (New York: Deutscher Weckruf und Beobachter, 1936), R.G. 131, Box 18.

[6] In the late 1930s the German American Bund had the distinction of ranking first in order of importance of all anti-Semitic organizations in the United

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The Bund's hysterical anti-Semitism was revealed in the 1936 presidential election campaign. The Jewish question was identified as the voters' central concern. Bundists charged that Roosevelt was dominated by a closed inner circle of Jewish Communists, facetiously called the Brain Trust. A *Deutscher Weckruf* cartoon of the administration placed the names of Roosevelt's Jewish or allegedy Jewish advisers in each section of a large Star of David. Biting comments followed each name: Secretary of Labor Frances Perkins, for example, was labeled "a friend of aliens" who favored Jewish Communist control over American labor. It was clear to Bundists that the New Deal was a "Jew Deal."[7] The *Deutscher Weckruf* stressed that German-American grievances against Roosevelt were far too numerous to list. Landon alone offered hope to a humiliated German community in America.[8] Kuhn believed that a Landon administration would inaugurate an aggressive anti-Communist crusade and establish warm commercial relations with Germany. He forbade members to support Roosevelt and any Jewish or Communist candidate.[9] After Roosevelt's great victory, the Bund engaged in post-mortems and issued dire predictions. Landon lost because he had solicited Jewish support and bowed to the pressures of the American Jewish community, and in a short time a Popular Front would emerge and evolve into a Communist dictatorship.[10]

The propagation of National Socialist racism infuriated American anti-Nazis. Contributing also to their anger were the Bund's other Nazi characteristics which Kuhn made no attempt to disguise. As in the NSDAP, the *fuehrerprinzip*, or leadership principle, underlay the basic organization of the Bund and gave Bundesfuehrer Kuhn the final authority over every sphere of the movement's affairs. From national headquarters in New York City's

States. Two sources take note of this development: Donald S. Strong, *Organized Anti-Semitism in America* (Washington, D. C., 1941) and Norton Belth, "Problems of Anti-Semitism in the United States," *Contemporary Jewish Record*, II (1939), 6-19.

[7] *Deutscher Weckruf und Beobachter*, Oct. 8, 1936, p. 8. Cited hereafter as *Deutscher Weckruf*.

[8] *Deutscher Weckruf*, Oct. 29, 1936, p. 1.

[9] *Bund Command Number 2*, Oct. 29, 1936, R.G. 131, Box 16.

[10] *Deutscher Weckruf*, Nov. 12, 1936, p. 1.

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German-American community of Yorkville, Kuhn controlled an organization divided into three *Gaus*, or departments: east, midwest, and west. The most typical subdivision was the city unit.[11] Although claiming members in every state except Louisiana, the Bund operated chiefly in northern and eastern urban centers, particularly in the New York metropolitan area.[12] The Bund always kept secret the actual number of members. At the peak of its power in 1937 and 1938 there were approximately 8500 members and 5000 to 6000 anonymous sympathizers.[13]

The most dedicated and fanatical fighters of the movement belonged to an elite group known as the Ordnungs Dienst (Order Service) or OD. Fully outfitted in fascist-style uniforms, the OD aped the SA. The Bund intended to draw its future leaders from OD ranks, and it specified a highly disciplined and rigorous training for each OD member. Even at the cost of his life an OD member was expected to defend the movement against internal and external attacks and commit himself completely to National Socialist principles.[14]

Inspired by the Hitler Youth, the Bund created a youth division. Bund youth members took German lessons, received instructions on how to salute the swastika, and learned to sing the "Horst Wessel Lied" and other Nazi songs. They wore uniforms, became familiar with the life stories of Nazi heroes, listened to Hitler's speeches on short wave radio, and were encouraged to correspond with Hitler Youth members.[15] Youth and OD

[11] *Constitution of the German American Bund*, R.G. 131, Box 104.

[12] *Kämpfendes Deutschtum* (New York: Deutscher Weckruf und Beobachter, 1938), 50. This is the Bund yearbook.

[13] During the late 1930s, questions concerning the composition of the membership rarely received satisfactory answers. Most observers, including journalists and politicians, believed that German nationals comprised the bulk of the members. In fact, German citizens accounted for about 25 per cent of the total; The remainder were native Americans and recently naturalized German Americans who felt strong ties to Germany. See Department of Justice, *German American Bund*, 19-23; Louis DeJong, *The German Fifth Column in the Second World War* (Chicago, 1956), 276; Strong, 29-30. Strong investigated Bund members in the Chicago area and concluded that they were generally of lower middle class origin.

[14] *The OD of the German American Bund* (n. p., n. d.), R.G. 131, Box 15 (Mimeo).

[15] The Youth Division published a monthly magazine called *Junges Volk*

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groups gave sensational displays of their devotion to Nazism at Bund camps, the two largest and most important of which were Camp Siegfried at Yaphank, Long Island, and Camp Nordland at Andover, New Jersey. During the summer months, thousands of people came to Siegfried and Nordland to attend noisy, belligerent, and joyous open air rallies. Accompanied by a marching band, fascist salutes, swastikas, stamping feet, rousing applause, thunderous "Sieg Heils," and Nazi songs, speakers harangued their audiences about Jews and Communists, anti-Nazi critics, and the need for a united front to save Germany and America.[16]

As its extreme ideology and characteristics were brought to public attention by the press, the Bund vaulted into national prominence and acquired a host of critics. This strong anti-Nazi activity played an important role in the movement's demise. No other foreign-inspired organization except the Communist Party aroused such resentment. Newspapers, veterans organizations, civic, religious, and labor groups directed scathing criticisms at the Bund. Fist fights and minor riots broke out when anti-Nazis attended Bund gatherings. Individuals initiated legal suits to control its affairs. In several states special legislative committees investigated American Nazi activities. Congressional concern over the Bund was in part responsible for the creation of one of the most controversial legislative committees in American history—the House Committee on Un-American Activities.[17]

The rising anti-Bund sentiment gained even greater significance

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appearing irregularly beginning in January 1937. See R.G. 131, Box 104 for the complete edition.

[16] A good account of a Nazi celebration at Nordland is *May Day Celebration in Honor of National Labor* (New York: Deutscher Weckruf und Beobachter, 1937), R.G. 131, Box 140. See John Roy Carlson, *Under Cover* (New York, 1943), 108-10 for a colorful report on a day at Camp Siegfried. Bund writers always emphasized the tremendous number of people who allegedly attended the mass rallies as well as the exuberant enthusiasm displayed, implying that the movement had an almost irresistible appeal to German Americans and even to other Americans who were "waking up to the dangers of Jewish Bolshevism." Accounts of rallies fill the pages of the Bund newspaper.

[17] The New York *Times* gave excellent coverage to nationwide Bund and anti-Bund activities. See August Raymond Ogden, *The Dies Committee* (Washington, D.C., 1945) for a discussion of the Bund's role in the establishment of the House Un-American Activities Committee.

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when official diplomatic circles in Washington and Berlin expressed mounting concern over Kuhn's organization. While American diplomats, particularly Prentiss Gilbert the United States chargé d'affairs in Berlin, complained to the German Foreign Ministry about the activities of certain German nationals in America, Hans Dieckhoff, the German ambassador in Washington, sent reports to Berlin stressing the damage wrought to German-American relations by the "stupid and noisy activities" of the Bund. He urged Reich agencies and German nationals to sever all connections with the Bund. The most important result of Dieckhoff's reports and the protests of other Foreign Office officials was Berlin's public announcement on March 1, 1938 prohibiting German nationals from membership in the Bund and the Citizens Protective League.[18] Although the Bund publicly minimized the importance of Berlin's decree, Kuhn went to Germany to appeal the decision. In a conversation with Fritz Wiedemann, Hitler's adjutant, Kuhn expressed astonishment over the German ruling, claiming that it would destroy his organization. Wiedemann refused to alter the decision and informed the Bundesfuehrer that his fumbling imitations of the Party had strained German-American relations. Undaunted, Kuhn returned to America bragging of warm receptions by Goebbels and Goering.[19] Kuhn's insincere boasting may have elevated his position within the move-

[18] William E. Dodd, *Ambassador Dodd's Diary, 1933-1938* (New York, 1941), 340; Remak, " 'Friends of the New Germany,' " 39; A. L. Smith, 92-102; Frye, 85-87; James V. Compton, *The Swastika and the Eagle* (Boston, 1967), 63-68; Manfred Jonas, *Isolationism in America, 1935-1941* (Ithaca, N.Y., 1966), 208-09. Clearly, the Bund had ties with Nazi propaganda agencies, particularly the Deutsches Ausland Institute (DAI), an organization concerned with the status of German nationals abroad. The story of the Bund's contacts with Nazi party and propaganda agencies is tangled; the Germans officially denied Reich connections with the Bund although the evidence shows such links were maintained. Generally, Dieckhoff called for restraint while authorities in the various Nazi agencies, mistaking Kuhn's boisterous activities to be signs of the Bund's power and influence, often continued contacts with him in violation of official policy statements. But the erratic and surreptitious nature of these communications precluded the possibility of day-to-day German control over the Bund, and by 1939 they had dwindled to insignificance.

[19] New York *Times*, Mar. 2, 1938, p. 13; *Deutscher Weckruf*, Mar. 10, 1938, p. 1; Joachim Remak, "Germany and the United States 1933-1939" (Ph.D. dissertation, Stanford University, 1954), 192; Frye, 88.

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ment, but it was one of his errors which worked against Bund interests. American press accounts of his alleged meetings with Nazi bigwigs helped fix the public image of the Bund as a Nazi tool.

In facing a widening circle of critics, the Bund adopted a curious self-defeating policy. Hoping to soften public opinion, it dropped some of its most obvious Nazi trappings: the slogan "Free America" replaced "Sieg Heil"; patriotic American songs superceded Nazi favorites; the OD uniform was modified. At the same time the Bund presented a new American program aimed at extending its appeal beyond the German-American community. Officially announced at the Bund's 1938 national convention, this program was blatantly racist, calling for the establishment of a racial state similar to the Third Reich with rigid restrictions placed on the mobility of Jews and on their admission to important positions in all areas of power in American society.[20] In line with this American program, Kuhn instructed members to attract Americans to the organization through personal contacts and by distributing Bund publications.[21] Circulation of English language pamphlets welcoming all "patriotic Aryan Americans" to meetings and membership increased. Bund rallies were now tagged pro-American meetings, and Bundists themselves made more appearances at native fascist gatherings.[22]

The most dramatic and publicized display of American Nazism occurred on February 20, 1939, when 20,000 people met in Madison Square Garden to participate in "a monster demonstration for true Americanism." Banners with slogans "Wake up America—smash Jewish Communists," "1,000,000 Bund members by 1940" along with American and Bund flags decorated the Garden. All of the important Bund leaders gave short sardonic speeches, including references to "parasitical aliens," "Christ haters," and "Jewish

[20] *Minutes of the 1938 Convention of the German American Bund.* The author thanks Mr. J. Walter Yeagley, Assistant Attorney General, U.S. Dept. of Justice, for providing him with a copy of this document.

[21] *Bund Command Number 21*, Oct. 8, 1938, R.G. 131, Box 16.

[22] For leaflets and pamphlets stating the new goals of the Bund, see R.G. 131, Boxes 1 and 20. *Deutscher Weckruf*, Sept. 29, 1938, p. 6; Nov. 3, 1938, p. 1. New York *Times*, Oct. 31, 1938, p. 10.

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dadaists," to convey their message of hate.[23] Throughout the rally the crowd responded to the speakers' cues with appropriate boos and cheers. An allusion to the president as "Frank D. Rosenfeld" brought loud boos and catcalls while a reference to Father Coughlin elicited frenzied cheers and applause.[24] A survey of nationwide editorial comment at the time on the Madison Square Garden rally reveals unanimous condemnation of the shocking display of hate propaganda. The Bund was undismayed; it called the event a smashing hit which had demonstrated "unprecedented enthusiasm for Christian patriotic Americanism."[25] This failure to perceive the depths of American resentment toward their organization demonstrates the ineptness of Bund leaders to cope with a rising anti-Nazi sentiment, an ineptness which pointed the way to their downfall.

Equally damaging to their future was their continued justification and glorification of Hitler's moves on the Continent. Bundists rejoiced when Hitler won his great diplomatic triumph at Munich. Mass rallies in New York City and at Camp Nordland celebrating Hitler's victory over "Communistic Jewry" brought crowds to hear Kuhn thank the German chancellor for "liberating the Sudetens." There were no apologies for the Nazi pogrom of November 1938. The German government was simply forcing "the international nation of parasites" to pay for its crimes by striking at all Jewish Communists within Germany. Shortly after Germany invaded Poland in early September 1939 Bund leaders issued a statement demanding that America observe strict neutrality. They interpreted the Nazi-Soviet pact as an example of Hitler's ability to outwit a Jewish plot to encircle Germany. The alleged conspiracy failed in Russia with the dismissal of Litvinov; an alert Germany came to terms with Russia, not Jewish Marxism, and "knocked the Jewish British encirclement plan into a cocked hat." Bund

[23] *Six Addresses Delivered at Madison Square Garden* (New York, 1939), R.G. 131, Box 140.

[24] Alson J. Smith, "I Went to a Nazi Rally," *Christian Century*, LVI, Mar. 8, 1939, pp. 320-22; "Hessian Day Rally," *New Republic*, LXXXVIII, Feb. 22, 1939, p. 57;"New York Nazis Beat up a Jew at Bund Meeting," *Life*, VI, Mar. 6, 1939, pp. 22-23.

[25] "The German American Bund Meeting," *Contemporary Jewish Record*, II (1939), 53-58; *Deutscher Weckruf*, Mar. 9, 1939, p. 2; Mar. 16, 1939, p. 2.

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leaders proudly announced their sympathy for Germany's war effort but argued that this did not signify disloyalty to the United States because German Americans would continue to fight for a Gentile America free of all atheistic Jewish Marxist elements.[26]

Heaped upon the Bund's mounting pile of troubles was a series of legal difficulties. The most serious was the indictment of Fritz Kuhn in May 1939, which resulted from the joint efforts of Mayor Fiorello LaGuardia and District Attorney Thomas E. Dewey,[27] on a charge of embezzling Bund funds, including the proceeds of the Madison Square Garden rally. In facing this challenge the Bund should have exercised prudence and self-control. Instead, the editors of the *Deutscher Weckruf* went into a rage. They characterized the affair as a display of "Jew York democracy in action."[28] Kuhn issued a statement absolving himself of guilt and charging that Dewey's attack was an act of desperation instigated by Communists who worried about the Bund's successful campaign of public enlightenment.[29] In late September Dewey demanded that the bail on the May indictment be raised to $50,000. Kuhn was unable to raise this amount immediately and spent several days in jail. His followers quickly took the offensive by opening an extremely scurrilous campaign against Dewey. They referred to him as a political slave of "the hate-blinded desperate international Jews." Leaflets filled with anti-Semitic references were distributed to solicit funds for Kuhn's defense.[30] His trial opened in November amid wide publicity. The press gave extensive coverage to embarrassing testimony which revealed Kuhn's extramarital relations. Kuhn's defense eliminated several counts of the indictment for lack of sufficient evidence, but he was convicted for mishandling a few hundred dollars of Bund funds. In early

[26] *Deutscher Weckruf*, Oct. 27, 1938, pp. 4-5; Nov. 17, 1938, pp. 1, 4; Nov. 24, 1938, pp. 1, 4-5; Dec. 8, 1938, pp. 1, 3-4.

[27] New York *Times*, May 26, 1939, pp. 1, 24; May 27, 1939, pp. 1, 5. "Woes of a Fuehrer," *Newsweek*, XIII, June 5, 1939, p. 12.

[28] *Deutscher Weckruf*, June 1, 1939, p. 1; June 8, 1939, p. 1.

[29] *Statement of National Leader Fritz Kuhn of the German American Bund*, June 1, 1939, R.G. 131, Box 159.

[30] *German Americans! All Americans! Stand Up Against Persecution!* Bund leaflet, R.G. 131, Box 47.

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December Kuhn left New York for Sing Sing to begin a two and a half to five year prison term.[31]

In the weeks following the trial the Bund gave an impressive show of unity. The *Deutscher Weckruf* stressed Kuhn's innocence and asserted that he was the victim of a Jewish plot. The new Bundesfuehrer, G. Wilhelm Kunze, a native American, issued a public statement praising Kuhn's past management of the organization. He sent a Christmas message to Kuhn assuring him that any member would trade places with him were it possible. A New Year's proclamation referred to Kuhn's conviction as a "crucifixion" and announced that the Bund would fight for a Christian America and a "free Kuhn." The Bund's youth magazine made Kuhn a martyr to Jewish revenge, condemned for crimes he could not possibly have committed.[32]

The entire organization appeared devoted to him, yet, secretly, the national leadership had expelled Kuhn from the Bund.[33] Kunze never formally communicated the leadership's repudiation of Kuhn to the members. Instead, he concentrated on tightening security: no member should offer information to journalists; admission to full Bund membership would follow a thorough six month investigation of the prospective member; all citizens of foreign countries would be screened out of the organization; to check criticism members would stage more singing and athletic events and fewer political meetings; each unit would continue to celebrate Hitler's birthday, privately if necessary.[34] Most important, Kunze outlined new aims, which focused on the Bund's duty to

[31] For a complete transcript of the testimony, proceedings, and exhibits of the trial, see R.G. 131, Boxes 48 and 49. Dewey's role in prosecuting Kuhn is briefly discussed in Rupert Hughes, *Attorney for the People* (Boston, 1940), 334-36.

[32] *Deutscher Weckruf*, Dec. 7, 1939, pp. 1-2, 7; Dec. 14, 1939, pp. 1-2. *Statement of the German American Bund regarding the indictment, trial and so-called conviction of Fritz Kuhn*, Dec. 11, 1939, R.G. 131, Box 204; Letter of G. Wilhelm Kunze to Fritz Kuhn, Dec. 18, 1939, R.G. 131, Box 204; *Junges Volk*, Dec. 1939 and Jan. 1940, R.G. 131, Box 104.

[33] *Minutes of a Special Meeting of the Executive Committee of the German American Bund*, Dec. 6, 1939, R.G. 131, Box 16.

[34] *Bund Command Number 25*, Nov. 30, 1939; *Bund Command Number 26*, Dec. 15, 1939; *Bund Command Number 27*, Jan. 12, 1940; *Bund Command Number 27-A*, Jan. 15, 1940, R.G. 131, Box 16.

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defend the rights of German Americans and to demand American isolation from the European war.[35] This marked another significant shift of emphasis in ideology. Kunze was directing the movement away from the program adopted at the 1938 convention. Anti-Semitism was played down; the concern to woo Americans into the Bund received less attention; the attacks on Communism lacked the fervor of earlier years. As the Bund worked in an increasingly hostile environment and as America adopted a preparedness program, Kunze stressed themes of survival.

In its defense of German Americans the Bund was in fact fending off attacks on itself. It pictured assaults on the organization as persecutions of all German Americans. When congressional legislation excluded Bundists from work under the WPA, the editors of the *Deutscher Weckruf* saw history repeating itself and the rise of the same anti-German hysteria that during World War I had resulted in measures directed against German Americans.[36] The Bund's identification with American isolationism, although a respectable cause, presented problems. Opinion polls showed that the majority of Americans favored aid to Britain and viewed Hitler and Nazism with contempt.[37] But the Bund blamed Britain for causing and continuing the war and acclaimed Hitler's conquests in Europe. The war, while a battle of freedom for the German people and National Socialism, transcended national boundaries and was a great world struggle between the forces of decadent Jewish democracy and the armies of progressive National Socialism.[38]

[35] *Minutes of the German American Bund Executive Committee,* Feb. 17, 1940, R.G. 131, Box 16.

[36] *Free American,* May 30, 1940, pp. 1-2, 7; June 6, 1940, pp. 1-2, 7-8; June 13, 1940, pp. 1-2, 7. The name of the Bund newspaper became *The Free American and Deutscher Weckruf* with the Jan. 4, 1940, issue.

[37] Hadley Cantril, "America Faces the War; A Study in Public Opinion," *Public Opinion Quarterly,* IV (1940), 395, 401; Hadley Cantril, Donald Rugg, and Frederick Williams, "America Faces the War; Shifts in Opinion," *Public Opinion Quarterly,* IV (1940), 652.

[38] *Free American,* Mar. 13, 1941, pp. 1-2, 7-8; Mar. 27, 1941, pp. 1, 3-4; May 1, 1941, pp. 1, 7; May 8, 1941, pp. 1, 3. The Bund, of course, was merely reflecting the Nazi propaganda line. German propaganda sources such as Transocean services and the German Library of Information dwelt on anti-British themes, and the Bund readily utilized their publications. See Frye, 96, for a good listing of the themes stressed by Nazi propagandists in their efforts to mold American opinion during the period of neutrality, 1939-1941.

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However glorious Hitler's war appeared to Bundists, internal problems forced them to devote their attention to the more mundane task of surviving as an organization. Kunze was an uninspired leader who could not command the respect and obedience of the entire membership. Members were supposed to follow the Bundesfuehrer without question, but many refused to recognize Kunze as national leader while others ignored his orders. A crisis of confidence resulted, with members resenting the fact that they had little to say about the choice and powers of the leadership. One strong faction continued to support Fritz Kuhn and was bitter over the Bund's failure to appeal his case. Mrs. Kuhn sent letters to influential members in which she denounced the national leaders as scoundrels. Rival factions fought a comic battle for control of a Bund meeting hall. Factional strife forced the closing of Camp Siegfried. While dissension was dividing the organization into quarreling, schismatic groups, debts had to be met and legal cases fought. There was little money available, and members chafed over increased financial demands and repeatedly refused to send money to national headquarters. The problem was compounded by the fact that Kuhn had kept no records of his business transactions. Kunze failed to make the organization financially solvent and could only plead with members to sell Bund pins and extra newspapers and to contribute to a reconstruction fund. The end result of the continual bickering and financial chaos was a steady decline in membership.[39]

By the summer of 1941 the Bund could no longer hold public meetings. The local organizations masqueraded as either singing or gymnastic societies in order to avoid violence and harassment.

[39] *Minutes of the National Convention of the German American Bund*, Aug. 31-Sept. 1, 1940, R.G. 131, Box 2. Most critics of the Bund assumed that it received large financial outlays from the German government, but little evidence shows that strong financial links existed. Seemingly it received some support: Nazi propaganda material was acquired at bargain prices, the *Deutscher Weckruf* received some outside promotional funds and carried advertisements of German railroads and tourist agencies. The Bund, however, appears to have operated largely on its own resources and it managed to break even each year until the expense of legal battles threw its finances into chaos. Like the Nazi party in the early 1920s, its main sources of income were membership dues, admission to Bund events, and the sale of pins, insignia, and propaganda materials.

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The OD became an athletic organization.[40] Meetings of the executive committee were preoccupied with tedious legal problems such as the closing of Camp Nordland by New Jersey authorities and the inclusion of Bund funds in a general order freezing Axis assets in America.[41] The plight of the movement was hopeless, yet the Bund outwardly remained defiant. After German armies invaded the Soviet Union and the United States began lend-lease aid to Russia, the Bund, echoing Nazi propagandists, returned to its favorite theme—the Jewish Communist plot to take over the world. Roosevelt's commitments to "Bolshevized Britain" and Soviet Russia proved to Bundists that the war had become a crusade to make the world safe for Jewish Communism.[42] Kunze's directives to members, however, said nothing about a worldwide conspiracy. In a tone of desperation and self-pity be stressed "the increasing seriousness of the present political situation" and blamed local leaders for shattering the organization.[43]

In early November Kunze suddenly fled to Mexico. The executive committee immediately met in special session to formally depose Kunze and to appoint Midwest Gau leader George Froboese active national leader. Members were informed that Kunze was dismissed on grounds of incompetence.[44] Froboese tried to rally the disorganized members by affirming the absolute righteousness of the Bund's cause. Kunze may have lost faith in the movement's future and, in the history of the organization's struggles, many others lacking spiritual and physical strength may have also fallen, but the new Bundesfuehrer pleaded, "We must carry on."[45]

On the day after Pearl Harbor the executive committee unanimously adopted a motion to disband the Bund at some early date. Three days later, on December 11, 1941, the United States declared

[40] *Minutes of the National Convention of the German American Bund*, Aug. 30-31, 1941, R.G. 131, Box 2.

[41] *Minutes of the German American Bund Executive Committee*, Mar. 31, 1941, R.G. 131, Box 16.

[42] *Free American*, Sept. 18, 1941, p. 1; Sept. 25, 1941, p. 1; Oct. 9, 1941, p. 1; Nov. 6, 1941, pp. 1, 6; Dec. 4, 1941, pp. 1, 5.

[43] *Bund Command Number 47*, Sept. 25, 1941, R.G. 131, Box 16.

[44] Rogge, 115; *Minutes of a Special Meeting of the German American Bund Executive Committee*, Nov. 13, 1941, and *Bund Command Number 49*, Nov. 13, 1941, R.G. 131, Box 16.

[45] *Bund Command Number 50*, Dec. 2, 1941, R.G. 131, Box 16.

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war on the Axis Powers and Treasury Department agents raided Bund national headquarters and seized all records.[46] Long before Pearl Harbor, however, the Bund had ceased being an effective organization. The combined factors of internal decay and outside pressures had made the movement impotent even though the most resolute and zealous members remained dedicated to their cause and Americans continued to believe sensational reports about the Bund's strength and influence.

[46] *Minutes of the German American Bund Executive Committee*, Dec. 8, 1941, R.G. 131, Box 16; Rogge, 129; New York *Times*, Dec. 16, 1941, p. 31. The Federal government began an immediate round-up of leading Bund officials. Kunze was picked up in Mexico and returned to the United States. He received a fifteen-year prison sentence for engaging in subversive activities. George Froboese and a few lesser known Bundists committed suicide. Twenty-four officers of the Bund were convicted of conspiracy to violate the 1940 Selective Service Act. Kunze along with other Bundists were defendants in the famous sedition case *United States vs McWilliams*. Some Bundists had their naturalizations revoked and spent a few months in detention camps. Fritz Kuhn lost his citizenship and was deported to Germany after 1945. Most Bund members, however, were not unduly harassed. During the war a few spy cases involving ex-Bundists created sensational news copy but proved nothing about subversive activity among the mass of Bund members. See Department of Justice, *German American Bund*, 3-5; *Contemporary Jewish Record*, V (1942), 422, 522; Francis Biddle, *In Brief Authority* (New York, 1962), 239-43, 291-95.

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The Missile Gap Controversy*

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The disputes surrounding the formation of military policy are not famous for their clarity, yet, even so, the controversy over the missile gap stands out as a muddled issue. The controversy arose in the late 1950s as a result of intelligence estimates that between 1960 and 1964 the Soviet Union might have more intercontinental ballistic missiles (ICBMs) operational than would the United States. Assuming the existence of a missile gap, opponents of the Eisenhower administration argued the existence of a deterrence gap, that Soviet supremacy in ICBMs was so great that the American strategic forces could be eliminated in a single massive attack. Administration spokesmen generally conceded the missile gap but denied a deterrence gap on the grounds that the American strategic forces were too numerous and varied to be eliminated by a single attack and that the leaders of the Soviet Union were aware of this fact.

We now know that the administration's contention that a missile gap did not necessarily imply a deterrence gap was never tested, since the missile gap itself never developed. Therefore it

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is almost impossible to evaluate the question of the deterrence gap, and we shall not endeavor to do so here—although, as the administration quite rightly pointed out, this rather than the missile gap was the relevant issue.

The missile gap was the result of a deliberate decision by the Eisenhower administration. The United States had many more strategic bombers than did the Soviet Union.[1] However, the Russians had recognized before the United States did the importance of the ballistic missile as a delivery system for hydrogen weapons,[2] and they appeared to be ahead in developing this new weapon which might neutralize the American advantage in manned aircraft. It was obvious that the United States, to meet the challenge, would have to develop a ballistic missile; the question was what kind and how many.

The choice was between rockets powered by liquid or by solid fuels. The liquid-fuel missile could be operational first, but the solid-fuel missile would be less expensive, have a much faster response, and be easier to transport and harden. It was decided to develop both types as rapidly as possible but to produce only a minimum number of liquid-fuel rockets, concentrating production on the more efficient solid-fuel missiles. The problem was that if the Soviet Union concentrated on producing liquid-fuel ICBMs, it appeared that the USSR could achieve temporarily a large numerical advantage before the United States would begin to introduce its solid-fuel Minutemen and Polaris weapons in 1963 and 1964; this period of potential danger became known as the missile gap.

The determination of production figures for the United States thus depended on estimates of Soviet production, estimates of varying reliability. The national intelligence estimates were made by the US Intelligence Board composed of representatives from the major intelligence agencies. These estimates were usually concerned with capabilities—how many missiles the Soviet Union

[1] For a summary of the strategic force strengths during this period of the United States and the Soviet Union, see Lincoln P. Bloomfield, Walter C. Clemens, Jr., and Franklyn Griffiths, *Khrushchev and the Arms Race: Soviet Interests In Arms Control and Disarmament 1954-1964* (Cambridge, Mass., 1966), 93-98.

[2] Allen Dulles, *The Craft of Intelligence* (New York, 1963), 163.

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could produce if it mobilized all available resources. Unfortunately Russian capabilities were thus compared with American production, a comparison obviously exaggerating Russia's apparent strength. On the other hand, nothing else could be used to make projections of Soviet strength far into the future, which had to be done for items with long lead times, such as missiles.

Despite a great deal of public interest in the issue (in the late 1950s it was a major political question), not much solid information is available.

> What is disquieting about this debate is that the stands taken by opposing spokesmen cannot be verified by any known facts. The figures on past, present and future missile production, by this country and the U.S.S.R., are in the first place military "secrets" and in the second place, especially as regards future production, only estimates of greater or less reliability. . . . It all boils down to a matter of confidence in the one who makes the interpretation.[3]

A great deal of testimony was given to various congressional committees, but the published records are something less than helpful, as shown by this fragment of typical testimony on the relative ICBM strength of the United States and the Soviet Union:

> Senator CANNON. For a total of (deleted) ballistic missiles.
> At that time, Russia will have had a capability of (deleted) in (deleted) but you do not know how many in (deleted).
>
> General WHITE. That is correct.
>
> Senator CANNON. Now, if I summarized your testimony correctly, you say we will have no (deleted) in the time that Russia will have the (deleted) ICBMs. How many Atlas missiles will we have in (deleted)?
>
> General WHITE. (Deleted) and (deleted) by end of fiscal year 1962.[4]

Thus the observer confined to open sources must utilize, not the national intelligence estimates themselves nor the congressional testimony of experts about these estimates nor even usually the direct reports by congressmen, but journalistic reports based on

[3] "Debate Over Missiles," *Commonweal*, LXIX, Feb. 20, 1959, p. 532.

[4] US, Congress, Senate, Committee on Armed Services, Preparedness Investigating Subcommittee, *Hearings*, 86th Cong., 1st Sess., 1959, p. 105.

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leaks and rumors about this testimony. The unreliability of such material is obvious.

In the early 1950s, the United States became concerned about the Russian development of ballistic missiles. According to Allen Dulles, then director of the Central Intelligence Agency, this concern was primarily responsible for the initiation of the U-2 overflights in 1955, and after 1956, photographs from the U-2 became available, furnishing much needed "hard" intelligence upon which later estimates were based.[5] In 1956 Trevor Gardner, former Assistant Secretary of the Air Force for Research and Development, testified that reliable data indicated that the Soviet Union had been forging ahead of the United States in missile development since 1953.[6] The CIA had established in Turkey powerful radar sets able to monitor Soviet missile test firings at least by 1957, when the Soviets first claimed to have tested an ICBM.[7] However, despite this early activity, it is fair to say that real public concern dates from the fall of 1957, when the Soviet Union announced in quick succession the firing of an ICBM and the launching of Sputnik, the first artificial satellite. For the first time the American public suddenly realized that the Soviet Union had developed a technology comparable to their own. Immediately serious questions were raised: whether a possible technological missile lag existed, whether the United States was two or three years behind the Soviet Union in missile and space technology development. Administration spokesmen and supporters denied the lag.[8]

Allen Dulles was called to testify on the subject before the Preparedness Investigating Subcommittee of the Senate Armed Services Committee, chaired by Senator Lyndon Johnson and including among its members Senators Stuart Symington of Missouri and Henry Jackson of Washington. Immediately the problem of secrecy arose, for Dulles apparently followed his usual policy of

[5] Dulles, 195-96, 164.

[6] US, Congress, Senate, Committee on Armed Services, Subcommittee on the Air Force, *Hearings, Airpower*, 84th Cong., 2nd Sess., p. 1137.

[7] "Goldwater on the Gap," *New Republic*, CXLVIX, Dec. 21, 1963, p. 5.

[8] New York *Times*, Oct. 5, 1957, p. 1; Oct. 6, 1957, p. 1; Oct. 7, 1957, pp. 16, 26; Oct. 7, 1957, p. 26; and Nov. 3, 1957, IV, p. 6.

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not allowing his testimony to be published even after he had censored it.[9] Senator Symington said that Dulles's testimony revealed a "sad and shocking story"; perhaps more significant was the reaction of Senator Styles Bridges, a staunch Republican and an administration supporter, who spoke of "very unpleasant information" which would "shock any complacency out of various officials and the American public."[10]

In December 1957, an article appeared in *Newsweek*, apparently inspired by a leak, which claimed Dulles had estimated that the Soviet Union would have an operational ICBM in about two years,[11] a fairly sound prediction as it happened. There was some acrimonious discussion about the article in a Senate committee, with the clear implication that the administration had simply released the most optimistic portion of the testimony. Some support for this contention was offered by an article in 1959 which asserted that United States intelligence had originally estimated that the Soviet Union would need about eighteen months from its first firing in August 1957 to make its ICBMs operational. The estimates were that the Soviet Union was capable of having 10 operational ICBMs by early 1959, 100 by early 1960, and 500 by mid-1961, assuming that development was pushed as rapidly as possible and that the major technical problems were well in hand.[12] The hearing ended in late January 1958 with Senator Johnson asserting that the Soviet Union was ahead of the United States in ballistic missile development.[13]

However, during 1958 the feeling that the Soviet Union had a significant technological lead over the United States lessened as this country began to match the Russian space exploits. Explorer I, the first American satellite, was launched on January 31, 1958, and although it was much smaller than the Russian satellites it quieted many fears. After the United States launched into orbit an Atlas missile which broadcast a Christmas peace message from

[9] Senate, *Hearings, Airpower*, 1324.

[10] New York *Times*, Jan. 28, 1958, p. 1.

[11] "Russia's Missile Firepower . . . Cold Facts," *Newsweek*, L, Dec. 9, 1957, p. 59.

[12] Charles J. V. Murphy, "The Embattled Mr. McElroy," *Fortune*, LIX (1959), 242. See the Table at the end of this paper.

[13] New York *Times*, Jan. 28, 1958, p. 6.

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President Eisenhower to the world, the New York *Times* signalled the end of the missile lag controversy in an editorial: "In every respect, in weight and size, but especially in its instrumentation, the Atlas demonstrates that, after an initial lag due to Pentagon economics, the United States has not only caught up with the Soviets but has gone beyond them."[14]

With the decline of the missile lag question came the rise of the missile gap question. To state it simply, most of the participants in this debate assumed that the Soviet Union and the United States had about the same technological capabilities; the crucial problem was now production, not knowledge. In July 1958, published figures "based on U.S. intelligence estimates" were attributed to Joseph Alsop giving the expected ICBM strength of Russia and the United States as: 1959, 100 to 0; 1960, 500 to 30; 1961, 1000 to 70; 1962, 1500 to 130; and 1963, 2000 to 130.[15] In the same month Senator Symington announced that the estimates of the CIA were too low, according to "other information I had" (presumably Air Force intelligence estimates), but the CIA refused to change its figures and gave the same ones to the committee in August and December, although by this time Symington had taken his protests to the president.[16] By January 1959 pressure was mounting to increase the ICBM program from its original level, thirteen squadrons (ten missiles per squadron) of liquid-fuel ICBMs, nine Atlas and four Titan, to be operational by 1963. The administration agreed to add seven more Titan squadrons, raising the projected United States strength in 1963 to 200.

Later the same month Secretary of Defense Neil H. McElroy testified before the Senate Preparedness Investigating Subcommittee and submitted a set of estimates for Soviet missile capabilities for the next few years. He also enunciated the administration's position on the whole question of estimates of Soviet capability

[14] *Ibid.*, Sept. 22, 1958, p. 14.

[15] Thomas R. Phillips, "The Growing Missile Gap," *The Reporter*, XX, Jan. 8, 1959, p. 11.

[16] US, Congress, Senate, Committee on Armed Services and Committee on Aeronautical and Space Science, *Joint Hearings, Missile and Space Activities*, 86th Cong., 1st Sess., 1959, pp. 25-26.

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and American missile programs in an exchange with Senator Symington:

> Secretary McELROY. It is not our intention or policy to try to match missile for missile in the ICBM category of the Russian capability in the next couple of years. Our position, Mr. Symington, is that our diversified capability to deliver the big weapon is what we are going to count on as our ability to deter general war.
>
> Senator SYMINGTON. What you are in effect saying is that we are planning in this fashion in spite of our intelligence estimates that the Russians are going to produce a great many more missiles, ICBMs, than we are; is that correct?
>
> Secretary McELROY. We are planning based on an assumption that they have that capability and *if they use that capability—and we are also assuming that they will from the standpoint of our counterforce* —then they will have a year from now, 2 years from now, a larger number of this particular element in the retaliatory force than the United States will have.[17]

On the same point, McElroy, at Symington's request, later read into the committee record a statement which he had made earlier at a classified hearing before the Senate Armed Services Committee: "I would like to make a final comment because I think it should be understood that from the standpoint of the Department of Defense we are assuming, as I think we should assume, that they [the Soviet Union] will have these numbers in being at the times when the national intelligence estimates say they could have it."[18] It thus appears from the available evidence that the Eisenhower administration was basing its defense programs on the assumption that the USSR would in fact fulfill its capabilities as these were assessed in the national intelligence estimates.

The second question was what the administration estimated the Soviet capabilities to be. Sometime before this, McElroy apparently stated publicly that by 1962 the Soviet Union, if it worked at maximum capacity, could have three times as many ICBMs as the United States.[19] Apparently this ratio was main-

[17] *Ibid.*, 46 (emphasis added).

[18] *Ibid.*, 47.

[19] There is some question as to whether the statement was ever formally made. I was unable to discover its source, and in 1960 Secretary of Defense

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tained in McElroy's testimony in January 1959. It was not, however, accepted by all concerned. Senator Symington felt that 4 to 1 was a more realistic estimate[20] and one author suggested it was closer to 6 to 1.[21]

McElroy's estimate was clearly a reduction from earlier ones.[22] It was apparently based on the fact that the Turkish radar sets had detected a long pause in the Russian ICBM testing program; intelligence officers interpreted this to mean that the Soviets were having technical problems and the intelligence estimates were modified accordingly. Although, as usual, no numbers were made public, the consensus among journalists seems to have been that the new estimates credited the Soviet Union with a capability of 100 ICBMs by 1960, 300 by 1961, 500 by 1962, and 1000 to 1500 by 1963.[23] Not all the journalists accepted these estimates; the Alsop brothers claimed that "qualified experts" (presumably again Air Force intelligence) said that more realistic figures might be as high as 300 in 1960[24] and 1000 by 1961.[25] Senator Symington said that he wanted the record to show that he had predicted that the Russians would have 3,000 ICBMs in three years,[26] or

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Thomas Gates said that his department had done research on the question and had been unable to locate it. However, he admitted that he had the "impression" that McElroy had made the statement. In view of the fact that McElroy has never denied it, although it has been quoted widely, he probably made it at a background briefing or similar off-the-record situation. See US, Congress, Senate, Committee on Armed Services, Preparedness Investigating Subcommittee and Committee on Aeronautical and Space Science, *Joint Hearings, Missiles, Space, And Other Major Defense Matters*, 86th Cong., 2nd Sess., 1960, pp. 477-78.

[20] US, Senate, *Joint Hearings, Missiles And Space Activities*, 220.

[21] Frank Gibney, "The Missile Mess," *Harpers*, CCXX (1960), 38.

[22] US, Congress, House, Committee on Appropriations, *Hearings, Department Of Defense Appropriations For 1960*, 86th Cong., 1st Sess., 1959, p. 829.

[23] Stewart Alsop, "Our Gamble With Destiny," *Saturday Evening Post*, CCXXXII, May 16, 1959, p. 117; "Missile Gap—How Perilous?" *Newsweek*, LIII, Feb. 9, 1959, p. 23; James R. Shepley, "Life-And-Death Debate Over Missile Programs," *Life*, XLVI, Mar. 9, 1959, p. 123; Murphy, 242.

[24] New York *Times*, Dec. 6, 1959, p. 1.

[25] Alsop, "Our Gamble With Destiny," 117. Later in the year the Alsop figures dropped somewhat, to 100 in 1960, 500 in 1961, 1000 in 1962, and 1500 in 1963; Washington *Post*, Oct. 7, 1959, p. A17. Note that the differences between these estimates and the national ones remain significant.

[26] New York *Times*, Mar. 16, 1959, p. 2.

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by 1962. Moreover, even those who accepted the government's estimates did not agree with its conclusion that the United States did not face a deterrence gap. One journalist stated the danger as it appeared to many: "At least until then [mid-1963], the Soviets will have a sufficient number of ICBM's to knock out most of SAC and the majority of our missiles in a single massive sneak attack."[27]

In January 1960, the new Secretary of Defense, Thomas Gates, testified before congressional committees and revealed what he called a new method of evaluating intelligence on Russian missile strength:

> Heretofore, we have been giving you intelligence figures that dealt with the theoretical Soviet capability. This is the first time that we have had an intelligence estimate that says, 'This is what the Soviet Union probably will do.' Therefore, the great divergence, based on figures that have been testified to in years past, narrows because we talked before about a different set of comparisons—ones that were based on Soviet capabilities. This present one is an intelligence estimate on what we believe he probably will do, not what he is capable of doing.[28]

The national estimates again dropped radically. Popular news magazines estimated them as 100 to 150 by 1961 and 400 to 500 by 1963.[29] This generally corroborated Symington's contention that the figures for 1961 were reduced in February to 34 per cent of what they had been in December 1959.[30] Gates indeed had suggested that the missile gap might not develop at all, although it certainly remained a likely possibility. There was a good deal of controversy over this new method of intelligence evaluation. Gates had worded his statement badly; it was not difficult to interpret it to mean that the estimates were based upon attempts by the administration to read the minds of the Russian leaders. Moreover, it was an election year, and the missile gap was a hot issue.

[27] Shepley, 123.

[28] US, Congress, House, Committee on Appropriations, *Hearings, Department Of Defense Appropriations For 1961, Part 1*, 86th Cong., 2nd Sess., 1960, p. 23.

[29] "The Coming Missile Gap," *Time*, LXXXV, Feb. 8, 1960, p. 18; "The Real Issue Behind Confusion Over Defense," *Business Week*, Feb. 13, 1960, p. 28.

[30] Stuart Symington, "Where the Missile Gap Went," *The Reporter*, XXVI, Feb. 15, 1962, p. 22.

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Critics charged that intelligence estimates had been doctored through using a subjective method of evaluation in order to keep defense expenditures low. Even Hanson Baldwin, military editor of the New York *Times* and a firm supporter of the administration before and after this event, said that Gates was on "slippery ground" when he used intentions instead of capabilities to predict Russian actions.[31]

The opposition to Gates's position increased after Allen Dulles again testified in closed session. Senator Jackson said that the situation was grimmer than Gates had indicated; Symington said that Dulles's testimony confirmed Symington's theory that the Soviet Union would have a 3 to 1 missile superiority by 1962.[32] Gates had said that the Soviets would have a "moderate" advantage in ICBM strength, but Senator Johnson found it difficult to reconcile this statement with Dulles's testimony and remarked that Gates's statement contained "an interesting use of the English language."[33] Later testimony by Major General J. H. Walsh, head of Air Force intelligence, revealed that the Air Force had dissented from the national intelligence estimate of Russian missile strength on the question of intent and capabilities.[34]

It seems fairly clear that the national intelligence estimates were in fact based upon hard facts and not just wishful thinking. The major reason for their change was the conclusion that the Soviet Union was not yet engaged in a crash program. Harry Howe Ransom suggests that this conclusion may have been based upon intelligence gained from the U-2 flights,[35] and it appears that as late as May 1960 the United States had positive intelligence about only two ICBM bases in Russia.[36] Further information apparently came from the radar sets in Turkey; Baldwin claimed that the

[31] New York *Times*, Jan. 22, 1960, p. 4.

[32] *Ibid.*, Jan. 30, 1960, p. 1. Symington seems to have reduced his estimate from the 4 to 1 quoted above.

[33] *Ibid.*, Feb. 2, 1960, pp. 1, 16.

[34] US, Senate, *Joint Hearings, Missiles, Space, And Other Major Defense Matters*, 115-16, 120-21.

[35] Harry Howe Ransom, *Can American Democracy Survive Cold War?* (Garden City, N. Y., 1963), 165.

[36] David Wise and Thomas B. Ross, *The U-2 Affair* (New York, 1962), 5; New York *Times*, Jan. 23, 1962, p. 12.

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Russian 5000-mile test shot in January 1960 was only the second test of that distance.[37] Thus it appears that the national estimates were based upon the actions of the Russians rather than their intentions as interpreted by Washington. Gates himself said: "There is obviously no intelligence whatsoever on U.S.S.R. intentions as to specific military or political policies or actions. Of course, it is impossible to have such intelligence. What we have is a refined and better set of facts pertaining to the probable or what the Soviet ICBM may be [*sic*]."[38]

However, even if the national intelligence estimates were accepted, a sizeable missile gap remained, and Henry Kissinger could write in 1960: "For all the heat of the controversy, it is important to note that there is no dispute about the missile gap as such. It is generally admitted that from 1961 until at least the end of 1964 the Soviet Union will possess more missiles than the United States."[39] The New York *Times* claimed both the national and Air Force estimates gave the Soviet Union a 3 to 1 advantage in ICBMs until 1963;[40] Gates put the period of maximum Soviet superiority in 1962.[41] Here the debate stayed for the remainder of the year, with the issue being used by the Democrats in the 1960 presidential campaign; Symington claims that Kennedy referred to it at least nine times during campaign speeches that have been reprinted.[42]

Immediately after his inauguration, President Kennedy asked Secretary of Defense Robert McNamara to undertake intensive studies of the defense situation; the committee examining the strategic scene was headed by Charles J. Hitch, the new Pentagon comptroller. Early in February 1961, McNamara gave a background briefing to a large group of newsmen. The New York *Times* reported that he had said there was, at the time, no missile gap but that he was uncertain of the future.[43] There was an im-

[37] New York *Times*, Jan. 23, 1960, p. 12.

[38] Testimony to House Committee on Science and Astronautics, quoted in Ransom, 164.

[39] Henry A. Kissinger, *The Necessity For Choice* (New York, 1960), 15.

[40] New York *Times*, Feb. 24, 1960, p. 13.

[41] US, Congress, Senate, Committee on Appropriations, *Hearings, Department Of Defense Appropriation For 1961*, 86th Cong., 2nd Sess., 1960, p. 5.

[42] Symington, 21.

[43] New York *Times*, Feb. 7, 1961, p. 1.

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mediate uproar, with Republicans charging that the entire issue had been fabricated by the Democrats to win the election and that it was now being discarded. McNamara retorted that the St. Louis *Post-Dispatch* had reported his statement correctly when it asserted "with complete authority" that he had not denied the existence of a missile gap. The *Times* was visibly miffed at the slur on its vaunted reportorial accuracy.[44]

In March 1961, the New York *Times* reported that General Lyman Lemnitzer, Chairman of the Joint Chiefs of Staff, had testified that Army and Navy intelligence were estimating Soviet missile strength as of mid-1961 at about 50 ICBMs, while the Air Force contended that the proper figure was 200.[45] The figure 50 was repeated in other sources,[46] and it fits Symington's contention that by June 1961, the national estimates had been reduced to 15 per cent of what they had been in December 1959.[47] The New York *Times* also estimated current United States ICBM strength at 27;[48] apparently the missile gap had finally appeared, although hardly in the anticipated magnitude. It is probably fair to regard this as the effective end of the issue; later developments simply established the superiority of the United States in ICBMs and raised questions as to the motives of those who had put forth the issue in the first place.

What should we conclude from the missile gap controversy? On the surface the lesson is obvious. The administration has secret intelligence information not available even to those members of Congress who have access to a good deal of secret data; this gives the executive a tremendous advantage in dealing with defense and national security matters. However, since this information cannot be released to the public, the administration may be unable to reply to criticism from politically motivated individuals and/or concerned citizens. Therefore in any such controversy the ad-

[44] *Ibid.*, May 13, 1961, p. 4.

[45] *Ibid.*, Mar. 1, 1961, p. 6.

[46] "The Missile Gap Flap," *Time*, LXXXVII, Feb. 17, 1961, p. 12; "Was There Ever A 'Missile Gap'?—Or Just An 'Intelligence Gap'?" *Newsweek*, LVIII, Nov. 13, 1961, p. 23.

[47] Symington, 22.

[48] New York *Times*, May, 12, 1961, p. 1; cf. Bloomfield, Clemens, and Griffiths, 96.

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ministration should at least be given the benefit of the doubt, and criticism should generally be discouraged.

However, we may legitimately question whether the administration actually performed as well as might be thought. One widely held theory has been that the administration had information, probably from the U-2 flights, which enabled it to predict accurately that the Soviet Union would not produce missiles as rapidly as possible and that therefore the capability estimates of the national intelligence board could be safely disregarded.[49] This may be true, but the evidence on the public record hardly supports it. Secretary McElroy was questioned closely on this very point and he said he believed that planning must be based on the assumption that the Russians would work to their maximum capabilities. Allen Dulles described the typical process of making intelligence reports: "As long as a Soviet system is still in its early stages, our estimates will stress capabilities and probable intentions; as hard facts become available it is possible to give an estimate of the actual programming of the system."[50] As hard information came in, from the U-2 planes, from the Turkish radars, and from other sources, it was apparently incorporated into the capability estimates, generally decreasing them. However, this meant that each set of capability estimates was realistic and that the administration's program for that time was formulated in terms of these estimates.

There is one remaining possibility which cannot be ruled out. If the administration actually had intelligence which proved that the Soviet Union would not introduce a crash program but which was so secret that it was restricted to a very small circle of top decision-makers, it might have based its programs on this information but have allowed the capability estimates to stand unchanged. This is a possibility, but the burden of proof rests with those who make the claim. It seems clear that the U-2 plane was not such a source, for after its exposure in May 1960 there was no drastic change in national intelligence capability estimates. A spy working on the alleged level of Oleg Penkovsky might have been such a source, although it seems unlikely that so much would be

[49] "Was There Ever A 'Missile Gap'?" 23.
[50] Dulles, 162.

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staked on any one man; apparently it was not Penkovsky himself, since the available materials indicate that he did not pass information to the West until April 1961,[51] after Eisenhower left office.

In any event, we must consider the question raised by Herman Kahn: if the Soviet Union actually had launched a crash ICBM program, as it appeared they might, would the United States have reacted in time?[52] This suggests two other questions: how much time would it have had to act, and would a missile gap have made any difference. Apparently it would have taken about thirty months to build missile bases and train the necessary personnel, assuming that these were done simultaneously.[53] It seems clear that if the United States had been given adequate warning it could have mobilized its resources and taken steps such as greatly increasing bomber production and constructing additional airfields, as well as putting its own missile production into high gear. Such warning could have been given by U-2 photographs; Francis Gary Powers was reportedly on such a mission when he was shot down.[54] If the Russians had timed their program to begin just after a United States budget had been approved, the administration might have been forced to reveal its intelligence sources to get the massive supplementary appropriations needed for the work to begin immediately, but the situation probably could have been handled without too much trouble.

However, the problem becomes more complex if we do not assume such effective warning. The U-2 was shot down in May 1960 when capability estimates still conceded the Soviet Union a possible 3 to 1 advantage for the next two or three years, with 100 to 150 ICBMs by 1961 and 400 to 500 by 1963.[55] If the USSR had immediately commenced a crash program, would the

[51] Oleg Penkovsky, *The Penkovsky Papers* (Garden City, N. Y., 1965), 4.

[52] Herman Kahn, *On Thermonuclear War* (Princeton, 1960), 201.

[53] US, Senate, *Joint Hearings, Missiles and Space Activities*, 93-94; cf. US, Congress, Senate, Committee on Foreign Relations, *United States Foreign Policy: Developments In Military Technology And Their Impact On United States Strategy And Foreign Policy*. A Study Prepared by the Washington Center for Foreign Policy Research, The Johns Hopkins University. No. 8. 86th Cong., 1st Sess., 1959, p. 56.

[54] Wise and Ross, 5.

[55] See above, pp. 608-09.

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United States have received enough hard information in time to take compensatory action? There was certainly a good deal of sentiment in Washington that U-2 flights should be continued because of the lack of intelligence concerning Russian missile bases.[56] It is not clear what other means of gathering this information were available. One source claims that the first experimental launching of the Samos reconnaissance satellite took place on January 31, 1961, that the second one did not go into orbit until the fall of that year, and that thus effective intelligence did not come in until almost a year after the election,[57] which was a year and a half after the U-2 was shot down. The Penkovsky information did not come in until a year after the cessation of overflights, and by its nature it could not have been anticipated.

The other question, of course, is whether a substantial missile gap might also imply a deterrence gap. This is a complex question which cannot really be answered; suffice it to say that responsible military opinion was deeply divided on the issue, and the question is now answered by implying malicious motives to those on either side.[58] It does seem clear that the decision to risk a missile gap was a gamble, especially since the crucial point was not whether it would have allowed the Soviet Union to actually eliminate United States retaliatory forces but whether Soviet decision-makers might think that it would. Stated in these terms the decision recalls Kahn's definition of a calculated risk as a risk which cannot be calculated. The Eisenhower administration decided that the risk was worth taking, took it, and in the end never had to face its ultimate test.

[56] "The U-2 Must Fly Again," *U.S. News & World Report*, XLVIII, June 27, 1960, pp. 44-49.

[57] "Goldwater And The Gap," 6.

[58] One journalist pointed out that there were only forty-three SAC bases in 1959; Shepley, 120.

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Table: Summary of Estimates of Soviet ICBM Strength

Date	*Source*	*1959*	*1960*	*1961*	*1962*	*1963*	*% of 1961 Symington*
Nov. 1957	N.I.E.[a]	10	100	500	—	—	
Jun. 1958	Alsop	100	500	1000	1500	2000	
Jan. 1959	N.I.E.	—	100	300	500	1000-1500	100%
Mar. 1959	Symington	—	—	—	3000	—	
May 1959	Alsop	—	300	1000	—	—	
Oct. 1959	Alsop	—	100	500	1000	1500	
Jan. 1960	N.I.E.	—	—	100-150	—	400-500	34%
Mar. 1961	Air Force Intelligence	—	—	200	—	—	
Mar. 1961	N.I.E.	—	—	50	—	—	15%
Actual[b]	USSR	0	30	50	100	150	
	USA	0	0	75	250	400	

[a] N.I.E. = national intelligence estimates based on journalistic sources.
[b] Bloomfield, Clemens, and Griffiths, 95.

Note: Very little confidence can be placed in these figures; the best that can be said is that apparently there are no others available in open sources. Not only are their sources varied and unreliable, the sources themselves are not internally consistent. The gravest weakness is in Symington's assertion that in September 1961, the figures for mid-1961 were further reduced to 3.5 per cent of the January 1959 estimates (Symington, p. 22; cf. Jeremy J. Stone, *Containing the Arms Race: Some Specific Proposals* [Cambridge, Mass., 1966], 95). According to these figures this would represent a drop to about 12 missiles; no confirmation appears elsewhere in the open literature. There are three clear possibilities. Symington may be wrong, which would be convenient but cannot be assumed. The estimate may have dropped to 12 ICBMs and simply not have been reported in open sources; with the advent of the Kennedy administration, most of the earlier Democratic critics had lost their major motive to publicize such figures. But there is little reason to credit Republicans with any higher principles, and if they had leaked the figure it would have made the Democrats look even more ridiculous for previously advocating the missile gap theory. The third possibility is that Symington's 3.5 per cent refers to the figure of 50 missiles. In this case our entire scale is wrong, since the January 1959 estimate of the missile strength in 1961 should then be 1400 to 1500. The reader may take his choice of alternatives.

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The Application Clause of Article Five

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Article Five of the United States Constitution provides that amendments may be proposed either by a two-thirds vote of each house of Congress or by a national convention which can be assembled when Congress receives applications from two-thirds of the state legislatures. To date Congress has proposed all twenty-five amendments to the Constitution, the application clause serving only to stimulate congressional formulation of the Seventeenth Amendment.[1] Nevertheless, the House of Representatives has reported that between 1789 and 1961 the states have submitted 226 applications for a constitutional convention. These applications cover: direct election of senators, limitation on federal taxing power, prohibition of polygamy, general revision of the Constitution, world federal government, repeal of the Eighteenth Amendment, presidential tenure, treatymaking, taxation of federal and state securities, protective tariff, wages and hours, gasoline tax, tidelands problem, control of trusts, grants-in-aid, popular ratification of amendments, revision of Article Five, and the

[1] Five congressional proposals were not ratified by the states. These concerned: apportioning the House of Representatives (1789); compensating members of Congress (1789); permitting American citizens to accept foreign titles of nobility (1810); recognizing slavery where it existed in the states (1861); regulating child labor (1924).

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Townsend Plan.[2] Since this digest was issued, a new category, protests over recent Supreme Court decisions requiring legislative reapportionment,[3] has motivated thirty-three states to register forty-four petitions.[4] In view of these past and present efforts to amend the Constitution through the application clause, I propose to examine chronologically the convention mode in order to explain why it has not been successfully initiated.

The Precedent of Legislative Proposal

Originating in several early state constitutions,[5] the application clause of Article Five was included in the Constitution by the Convention of 1787 as a compromise between advocates of states rights and advocates of strong central government. The former objected that providing for amendments solely by congressional proposal would give the national government too much power,[6] while the latter delegates believed that the application mode would permit the states to subvert the purpose of the new constitution.[7] As a consequence both sides vacillated on the question, the clause being adopted on August 30, deleted on September 10, and then reinstated on September 15 in its present form. Throughout the debate, however, the only concern with implementation was voiced by James Madison, who perceived some of the problems not elucidated in the amending provision such as "How was a convention to be formed? By what rule decide? What [will be] the force of its

[2] Cyril F. Brickfield, *State Applications Asking Congress to Call a Federal Constitutional Convention*, House Judiciary Committee, 87th Cong., 1st Sess., 1961, p. 7.

[3] This reaction has been directed against the cases of *Baker v. Carr*, 369 U.S. 186 (1962) and *Reynolds v. Sims*, 377 U.S. 533 (1964).

[4] Counting other applications, the unofficial number of petitions submitted through 1969 is 317.

[5] Georgia, Pennsylvania, and Massachusetts.

[6] For example, George Mason believed that unless there was an alternative to legislative proposal, amending the Constitution might be impossible because Congress conceivably could "abuse their power and refuse their consent on that very account"; Max Farrand, *The Records of the Federal Convention of 1787* (New Haven, 1911-1937), I, 203.

[7] Alexander Hamilton anticipated that without congressional control of the amending process, "The State Legislatures will not apply for alterations but with a view to increase their own powers"; *Ibid.*, II, 558.

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acts?"[8] Since he opposed cluttering the Constitution with details, Madison showed little enthusiasm for allowing proposal by a national convention because, in his opinion, once it was convened, unnecessary difficulties could result from arguments over procedural matters.[9] Unquestionably, Madison's questions and many other unanswered ones have almost made the application clause a dead letter, but in the first instance of amending the Constitution, political considerations instead of procedural uncertainties persuaded Congress to select legislative over convention proposal to fulfill the famous "gentleman's agreement" of adding a bill of rights to the Constitution as a condition for securing its ratification in several states.

The principal architect of this compromise and the major force behind its introduction in Congress, James Madison, feared the consequences of a second national convention. A review of his correspondence during this period reveals a constant concern with satisfying the agreement by the most expeditious means while minimizing opportunities for the expression of dissident views.[10] It was his conviction that: "The object of the Anti-Federalists is to bring about another general convention, which would either agree on nothing, as would be agreeable to some, and throw everything into confusion, or expunge from the Constitution parts which are held by its friends to be essential to it."[11] In addition, Madison believed that most state legislatures were favorably disposed toward the new government.[12] It has been demonstrated how the erroneous "connected modes concept" controlled the use

[8] *Ibid.*

[9] Madison, who authored the basic framework of Article Five, favored an application clause by which the states could require Congress to propose amendments; *Ibid.*, II, 559.

[10] Madison's attitude is developed in Philip L. Martin, "Madison's Precedent of Legislative Ratification for Constitutional Amendments," *Proceedings of the American Philosophical Society*, CIX (1965), 49-50.

[11] Madison to Philip Mazzei, Dec. 10, 1788; Gaillard Hunt, *The Writings of James Madison, Comprising His Public Papers and His Private Correspondence* (New York, 1900-1910), I, 316.

[12] On Nov. 5, 1788, Madison wrote George Washington: "Indeed, Virginia is the only instance among the ratifying states in which the Politics of the Legislature are at variance with the sense of the people expressed by their Representatives in Convention"; *Ibid.*, I, 302.

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of Article Five in the incipient constitutional theory of that period;[13] Madison obviously preferred introducing his proposals in Congress for reasons of political expediency, thus establishing a precedent which has endured except for the ratification of the Twenty-First Amendment by state conventions.[14]

Applications of the First Hundred Years

Before Congress proposed the first amendments to the Constitution, Virginia (1788) and New York (1789) applied for a national convention to consider adding a bill of rights in accordance with the "gentleman's agreement," but this movement quickly ended for Congress soon submitted proposals to the states. Following this experience there was no more concern with the application clause until the nullification crisis of the 1830s when South Carolina, the leading participant in the controversy, circulated among the states resolutions urging that "a convention of the States be called as early as practicable to consider and determine such questions of disputed powers as have arisen between the States of this Confederacy and the General Government."[15] However, only Alabama and Georgia responded favorably since most of the states decided that the suggested assembly could not be considered a convention under Article Five without specifying the terms of amendment.[16] Delaware, for example, replied that "such a convention of the States, if assembled, could have no such power as that set forth by the resolutions of South Carolina; that any other mode, therefore, must be repugnant to its [Article

[13] According to this interpretation, the modes of proposal and ratification were inviolably connected so that, for example, congressional proposal required legislative ratification. For a detailed explanation of this concept, see Martin, "Madison's Precedent," 47-52.

[14] The efforts to use convention ratification are discussed in my "Convention Ratification of Federal Constitutional Amendments," *Political Science Quarterly*, LXXXII (1967), 61-71.

[15] *Senate Journal*, 22nd Cong., 2nd Sess., 1833, p. 83.

[16] In his compilation Herman V. Ames shows that only Georgia made a proper application since the Alabama and South Carolina resolutions did not call for a convention to consider amendments; Ames, *Proposed Amendments to the Constitution of the United States During the First Century of its History*, 54th Cong., 2nd Sess., House Document 353, Part 2, 1897, p. 345.

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Five's] provision."[17] Moreover, the nature of South Carolina's proposal was interpreted as requiring "a convention of the people . . . not a convention of the states" as prescribed by the Constitution. Also, regarding the temper of the nation as overriding all other factors, political leaders generally viewed the times as being inauspicious for Congress to summon a national convention for proposing amendments. Unquestionably, much of this latter reaction can be attributed to the uncertainty expressed in the deliberations of several states over controlling a convention once it was assembled. The desire to avoid repeating 1787 when radical changes were made in the constitutional system was thus a primary influence in the rejection of South Carolina's resolutions.

The threat of Civil War in 1860 revived interest in the application clause, and six state legislatures and one state convention petitioned for a national convention.[18] In addition, nine members of Congress introduced resolutions requesting that a convention be called for the purpose of proposing amendments to avert national conflict. When these efforts failed to elicit much support, Virginia issued a call for assembling an informal convention. This body, consisting of 136 delegates from fourteen slave and seven free states, convened in Washington, D.C. on February 4, 1861, but the peace conference lacked constitutional authority since it was not instituted under the provisions of Article Five. Depending entirely upon the amity of some states as backing for its recommendations, the conference submitted a number of conciliatory amendments to Congress which received them with only cursory comment.

During the Civil War three more congressional proposals to call a national convention were made without attracting any attention. Recognizing that the unanswered procedural questions could be a deterrent, one resolution even attempted to correct this deficiency in the application clause by stipulating that the number in each state delegation would be equal to the number of its representatives in Congress.[19] Nevertheless, this alternative means

[17] *Senate Journal*, 22nd Cong., 2nd Sess., 1833, pp. 157-58.

[18] Illinois, Indiana, Kentucky, New Jersey, Ohio, Virginia, and the Missouri Convention.

[19] *Senate Journal*, 37th Cong., 3rd Sess., 1862, p. 24.

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for proposing amendments, which Congress would not consider despite national strife, failed its crucial test. Not even congressional initiation could overcome the suspicions regarding use of the mode. These suspicions were a more decisive factor than the legal problems in its defeat.[20] Yet, during the next two decades several efforts were again made in Congress to use the convention method especially when disputes arose over presidential elections, as in the case of 1876.

The experience of the first hundred years indicates that the application clause was regarded as an extraordinary power reserved for critical occasions demanding national deliberation of fundamental constitutional conflicts. Otherwise, congressional proposal was regarded as proper for amendments to make either structural constitutional alterations or those such as the Civil War resolutions where the issues had been resolved in another manner. Undoubtedly, this concept of the constituent power was strengthened by a fear of what unexpected course a national convention might take; this institution was widely treated in theory, using 1787 as a precedent, as being unrestrained, since it represented the sovereign people. By the end of the nineteenth century, however, a different interpretation began to develop, the application formula being reconsidered as a means of accomplishing changes which Congress is reluctant to undertake.

Twentieth Century Applications

In step with the progressive tone which permeated the American political system around the turn of this century was the reform movement advocating a constitutional amendment transferring selection of senators from the state legislatures to the electorate. Naturally, the Senate, which between 1894 and 1902 vetoed four such resolutions approved by the House, opposed changing the process by which its members had secured their seats, thus

[20] In view of the history of the nullification resolutions, it is understandable why the Confederate constitution would include a less complicated application clause. Upon petition of the legally assembled conventions of three states, the Congress was required to call a convention for the purpose of considering recommended amendments.

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provoking state legislative subscription to the alternative mode. Between 1893 and 1911, thirty-three states[21] filed seventy-three applications for this purpose with Congress.[22] Under pressure from virtually all quarters, the Senate finally acceded on May 13, 1912 to the House proposition that a year later became the Seventeenth Amendment.

To some degree the Senate's yielding was probably due to the state applications, which revived fears of a national convention. Another factor must have been the increasing political opprobrium attached to their rejection of a popular demand. Certainly, some efforts to stigmatize the opposition were made, as in 1910 when Democratic Senator Owen of Oklahoma accused the national Republican leadership of being the roadblock to passage of the proposed alteration.[23] A more logical answer, however, is that by 1912 the voters in at least half of the states had been granted the right to indicate preferences in the party primaries with the state legislatures generally adhering to these choices in the election of senators. Regardless of what motivated the Senate's decision, this experience changed the theory of how the application clause should be used because afterwards attempts have been made primarily to promote congressional proposal.[24] Consequently, constitutional objectivity has seldom been exercised in these efforts for some of them, despite claims of national concern, were clearly engendered by ideological or political motives which had limited appeal. For example, Connecticut in 1958 in an ostensible effort to protect its commuting population applied for a convention to study an amendment preventing taxation of a nonresident's in-

[21] Although not reported in the *Congressional Record*, the applications of Nebraska in 1893 and Wyoming in 1895 are listed in *Federal Constitutional Conventions*, Senate Document 78, 71st Cong., 2nd Sess., 1930.

[22] Cf. Brickfield, Appendix, Table 2, p. 17.

[23] *Congressional Record*, 61st Cong., 2nd Sess., 1910, XXXV, Part 7, p. 7113.

[24] During the period from 1906 to 1916 thirty petitions were submitted by twenty-seven states for a convention to propose an amendment prohibiting polygamy. Ten of these, including three reiterations, followed proposal of the Seventeenth Amendment. Furthermore, from 1899 to 1911 nineteen applications were made by fifteen states for a convention to consider general revision of the Constitution. However, the main interest of these cases was popular election of senators since fifteen of the resolutions also specified consideration of this alteration. Cf. Brickfield, Appendix, Table 2, p. 18.

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come by the state in which he worked.[25] Likewise, Texas in 1957 petitioned for consideration of an amendment regarding its peculiar tidelands problem with oil and mineral rights.[26] Another single application was also presented in 1957 by Alabama which, after the desegregation decisions affecting public education, proposed to gain control over the appointment of Federal judges.[27] By comparison few state memorials have been inspired by interpretations of national interest, although in the 1940s, eight petitions calling for the creation of a federated world government were received from six states while five states promoted the Twenty-second Amendment by advocating a limitation of presidential terms.[28]

Following adoption of the Seventeenth Amendment, the application process was used only in several cases of special interest during the next twenty-five years. The reaction against prohibition produced no more than five petitions since most of this movement concentrated on urging Congress to submit a repeal amendment to ratifying conventions.[29] In 1939, a campaign was begun to modify the Sixteenth Amendment when it became evident that Congress was unresponsive to the idea. Sponsored by the Committee for Constitutional Government, the "Millionaires Amendment," at the end of 1944, was supported by seventeen states, but this action was temporarily ended in the next two years when some congressmen and high ranking federal officials, frightened by the financial problems created by World War II, persuaded six states to withdraw their applications. Nevertheless, anticipating higher taxes because of the Korean conflict, opponents of the Sixteenth Amendment vigorously renewed their efforts with the result that seven states in 1951 applied for a convention. Again, criticism by influential congressmen and federal officers plus the opposition of some national organizations such as labor unions thwarted the drive for amendment.[30] Over the next several years

[25] *Congressional Record*, 85th Cong., 2nd Sess., 1958, CV, Part 6, pp. 8085-86.

[26] *Ibid.*, 85th Cong., 1st Sess., 1957, CIV, Part 6, p. 8265.

[27] *Ibid.*, Part 8, p. 10863.

[28] Brickfield, 18.

[29] *Ibid.*

[30] On both occasions criticism of the income tax applications sometimes became vitriolic with, for example, Representative Wright Patman of Texas call-

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six states rescinded their resolutions, and of the thirty-five petitions presented through January 1961 by twenty-eight states, twelve were eventually recalled, thus probably discouraging, for a few decades at least, any serious hopes of using Article Five to limit income taxation.

As noted earlier, the latest agitation to invoke the convention machinery has been excited by the Supreme Court's reversal of precedent in accepting legislative reapportionment suits initiated under the Equal Protection Clause of the Fourteenth Amendment. Following the *Baker v. Carr* decision in 1962, a campaign was organized by the Council of State Governments to bring before a national convention three amendments the effect of which would have been to reestablish a confederated government for the United States.[31] These propositions not only eliminated federal control over state legislative apportionment and restricted the power of the Supreme Court by requiring a review of its decisions before a Court of the Union comprised of the fifty state chief justices, but they also permitted the states to by-pass Congress in the amending process. Reminiscent of the Philadelphia debates on this subject,[32] this last recommendation changed the application formula by substituting for congressional and convention decision-making a procedure which provided that whenever a textually identical amendment was proposed by two-thirds of the state legislatures, it automatically would be circulated among the states for ratification by a three-fourths majority.[33] Pressured by effective propaganda eighteen states quickly joined in this protest and presented a total of twenty-nine petitions to Congress in 1963 re-

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ing them Fascist proposals and referring to a dishonorable experience of a leader in the movement. Cf. *Congressional Record*, 79th Cong., 1st Sess., 1945, LXXXXI, Part 6, pp. 7236-39; and 82nd Cong., 1st Sess., 1951, LXXXXVII, Part 8, pp. 10070-72.

[31] The pros and cons of these resolutions are analyzed in Charles L. Black, Jr., "Proposed Constitutional Amendments: They Would Return Us to Confederacy" and Frank E. Shanahan, Jr., "Proposed Constitutional Amendments: They Will Strengthen Federal-State Relations," *American Bar Association Journal*, XLIX (1963), 631-40.

[32] Farrand, *passim*.

[33] The implications of this amending provision are analyzed in Charles L. Black, Jr., "The Proposed Amendment of Article V: A Threatened Disaster," *Yale Law Journal* LXXII (1963), 957-66.

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questing that a convention be convened to consider one or more of the proposals.[34]

Despite this astonishing response, the movement was short-lived because as soon as its intentions were better understood, adverse public reaction made further advocacy politically unprofitable in many cases. However, after the Supreme Court in 1964 handed down its "one man-one vote" ruling in *Reynolds v. Sims*, the Council renewed its efforts urging support this time for an amendment which would enable the voters of a state through a referendum to apply the federal analogy to their legislature by having the representation of one house chosen on some basis other than population. Twenty-six states quickly responded with such applications to the Eighty-ninth Congress,[35] and counting two earlier resolutions, a total of twenty-eight were registered with Congress by the end of 1965.[36] At the same time that the states were being importuned to submit applications, Senator Everett Dirksen, representing thirty-seven other members, introduced a similar resolution in the upper house of Congress.[37] Although assisted by a newly created national organization which employed the famous California public relations firm of Whitaker and Baxter to manage the campaign,[38] this proposal, including a revised version,[39] twice failed by seven votes to secure the necessary two-thirds majority in the Senate. After the second defeat in 1966, the sponsors, refusing to give up, concentrated their ef-

[34] Twelve applications dealt with changing Article Five, twelve with legislative apportionment and five with the Court of the Union. Seven other proposals also made at this time, advocated reform of the electoral college and two concerned income taxation and the national debt. For an account of a separate but simultaneous move to reform the electoral college, see Fred P. Graham, "The Role of the States in Proposing Constitutional Amendments, *A.B.A.J.*, XLIX (1963), 1175-83.

[35] Of this number nine were resubmissions of applications made in response to the first campaign.

[36] For a discussion of these proposals and the uncertain status of four, see American Enterprise Institute, Special Analysis Series No. 5, *A Convention to Amend the Constitution?* (Washington, D. C., June 16, 1969), 13-16.

[37] S. J. Res. 2, 89th Cong., 1st Sess., 1965.

[38] These activities and senatorial reaction to them are described in the *Congressional Record*, 89th Cong., 2nd Sess., 1966, CXII, Part 3, pp. 3831-32 and Part 4, pp. 5153-58.

[39] S. J. Res. 103, 89th Cong., 1st Sess., 1965.

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forts on persuading several state legislatures to adopt the Council's amendment. This maneuver succeeded in gaining four more petitions in early 1967; and when Iowa was persuaded to join the movement in April 1969, a constitutional showdown seemed imminent as presumably the proponents were only one short of the requirement for calling a convention. The reapportionment of most state legislatures has evidently ended any chance for victory. Following the latest application, Kansas rescinded its request and similar efforts barely failed in Illinois, North Carolina, and Texas; moreover, convention supporters were subsequently defeated in the legislatures of Delaware and Wisconsin. The latter setback was particularly significant because in 1967 the Wisconsin Senate approved an application which seemed to fail only because it was sidetracked by the rush to adjournment. This time not even a dramatic appeal to the memory of Senator Dirksen, the guiding force in the convention movement until his death in the summer of 1969, could summon a majority in the Wisconsin Assembly.

Lacking constitutional and legislative guidelines or precedents, the validity of the thirty-three applications has consequently been questioned. To begin with, two that were made in 1963 have not been resubmitted which means Congress could conceivably disregard them as being part of the group associated with the latest resolution of the Council of State Governments. Secondly, since twenty-six of the petitions originated in malapportioned legislatures, Congress could invalidate them on the grounds that they do not represent the voice of the people.[40] Thirdly, while twenty-nine resolutions are almost identical in text, three request only the calling of a convention, raising the issue of whether the purpose of the amendment must be stated in an application for it to be valid. Problems such as these have induced several members of Congress, led by Senator Ervin of North Carolina, to seek solutions which will make the application clause a viable part of the Constitution. To this end he has introduced clarifying legislation

[40] Regarding this point, cf. *Congressional Record*, 90th Cong., 1st Sess., 1967, CXII, Part 8, pp. 10101-15.

On August 6, 1969 a Utah Federal District Court declared that state's application invalid because it had been adopted by a malapportioned legislature; See *Petuskey v. Rampton*, 307 Fed. Supp. 235 (1969).

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which, with revisions, stipulates that to assure validity all petitions must define the purpose of amendment and be filed with the Speaker of the House and the President of the Senate, who are to publicize fully their tabulations,[41] and these applications are to be valid for four years although subject during this time to rescission by the state legislatures which presented them. The Ervin Bill would make it very clear that when the requisite number of similarly categorized applications has been received, Congress must by concurrent resolution summon a national convention. The bill further recommends that each state's delegation equal in number the state's congressional representation and that the same selection process should be employed, two members elected at-large and the remainder chosen from the congressional districts. In order to properly represent the voting strength of each state in proportion to its population this scheme gives each delegate one vote, which means a total of 535 votes. The bill limits the convention to consideration of amendments under the subject or subjects indicated in the memorials. This is not to say, however, that a convention must confine its attention to specific amendments as outlined in the applications, but rather than it can deliberate all perceivable alternatives pursuant to the nature of the proposals. Enforcing this restriction is a corollary providing that whenever a convention violates its mandate by proposing amendments on other matters, then Congress can by joint resolution refuse to submit them for ratification, but Congress is forbidden to exercise such denial on the basis of substantive disagreement with a convention's decisions which conform to its stated purpose.

Although S. 2037 represents a commendable effort to implement the application clause, it failed for several reasons to gain approval of the Senate Judiciary Committee. On one hand some basic provisions could still be interpreted by Congress. For example, the categories of requested amendments could be narrowly defined either to exclude applications, putting obstacles in the way

[41] S. 2307, 90th Cong., 1st Sess., 1967. This bill was revised after hearings were held on October 30 and 31, 1967. It was introduced again (S. 623) in the 91st Cong., 1st Sess., 1969. A discussion of the arguments concerning this proposal is found in "Symposium on the Article V Convention Process," *Michigan Law Review*, LXVI (1968), 837-1016.

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of calling a convention, or to restrict severely the discretion of this assembly, making it essentially a ratifying body since Congress could reject any proposals inconsistent with its definition of jurisdiction. On the other hand, a more realistic explanation of defeat is that the supporters of S. 2037 could not dissociate their bill from the anti-reapportionment movement with which many of them were identified.

Conclusion

The experience of Article Five's application clause closely parallels the history of convention ratification[42] in that the use of both methods has been advocated mainly during periods of political and social conflict, and in most cases this involvement has been on the side of unpopular causes. Consequently, the recommendation of these alternative modes is often interpreted as being mainly for the purpose of protecting minority interests at the expense of democratic participation and is inevitably challenged by procedural questions regarding implementation although the practice of state constitutional revision and the ratification of the Twenty-first Amendment clearly demonstrate the mechanical feasibility of these methods. In any event, the record of the Twenty-first Amendment certainly indicates that unless there is strong public opinion favoring the convention formula, neither mode is likely to be used; and, furthermore, as evinced by the history of the Seventeenth Amendment, popularly supported applications will probably result in congressional rather than convention proposal of a desired amendment. In addition to these realities the application clause is also opposed by a persistent concern with the philosophical question of whether the power of a national amending convention can be limited to consideration of specific resolutions. Logically, the precedent of 1787 creates a fear that, once assembled, a convention might precipitate a constitutional crisis by reporting unanticipated changes in the American political system. Thus, for reasons of political suspicion, public disinterest, and theoretical uncertainty, the application clause will probably remain an unused part of the Constitution.

[42] Martin, "Convention Ratification."

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Book Reviews

The Improvement of Mankind: The Social and Political Thought of John Stuart Mill *by John M. Robson. Toronto, University of Toronto Press, 1968.—xii, 292 pp. $6.50.*

Virtually no other thinker of equal stature has been the victim of such inadequate scholarship as John Stuart Mill. The standard biography, Michael St. John Packe's vivid if overdramatic *Life of John Stuart Mill,* provides merely a superficial and sometimes inaccurate summary of Mill's leading ideas; and the only two important modern studies not confined to special aspects of Mill's thought, those of Karl Britton and R. P. Anschutz, suffer from brevity and make no claim to completeness. What Walter Kaufmann so brilliantly did for Nietzsche, no one has yet suceeded in doing for Mill.

On first glance John M. Robson's *The Improvement of Mankind* appears a step in the right direction. Drawing on an immense knowledge of texts and manuscripts relating to Mill, Robson correctly asserts that Mill's thought possesses a basic unity derived from utilitarian ethics. He convincingly lays to rest the myth that Harriet Taylor was "in any meaningful sense, the 'joint author' " (p. 68) of Mill's works. And he judiciously arbitrates the controversy over the extent to which Mill accepted socialism. Yet on the whole, *The Improvement of Mankind* is a disappointment. It suffers from errors in the chronology of Mill's development, frequent internal contradictions, an ambiguous style, and inept handling of crucial philosophical issues.

Though these faults pervade the book, they are most blatant in Robson's discussions of Mill's intellectual debts and his ethical theory. Robson wrongly asserts, for example, that "the influence of Coleridge was mainly confined to the decade 1830-1840." Actually, during the 1830s

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Mill was occupied with deciding which of Coleridge's insights were valid, and for the rest of his life he struggled to weave those insights into his thought. Robson himself points out at least three examples (pp. 71-72, 226, 229) of Coleridge's influence in *Representative Government* (1861)—which is itself an effort to solve problems of parliamentary representation first brought to Mill's attention largely by Coleridge. Moreover, in stating that "Carlyle introduced Mill to, rather than supplied, new modes of thought" (p. 94), Robson overlooks Mill's debts to Carlyle not only for his lifelong belief in "the organic and personal nature of truth" (p. 86) but also for many insights in "On Genius" (which Mill himself acknowledged) and in the two essays on poetry.

Robson's discussion of Comte's influence on Mill is equally inaccurate. He states that Comte's *Système* (1823) failed to leave a "distinct impression" when Mill first read it in 1828 and that consequently Mill did not realize the "strength of Comte's approach" until he read the *Cours* in 1837 (p. 95). Yet a few pages later Robson notes that in the early 1830s, Mill held at least one idea in common with "Comte and the Saint-Simonians" and that in 1835 "his agreement with Comte and the Saint-Simonians is marked" (pp. 100, 101). Besides contradicting himself, Robson is wrong that the *Système* had no effect on Mill, for Mill himself indicated its great impact in two letters to his friend Gustave d'Eichthal and in the early draft of the *Autobiography*. Robson is also incorrect in stating that in the early 1830s Mill thought "only those specifically qualified on social topics [the *pouvoir spirituel*] should be allowed not only to express, but even to form, opinions on matters of social importance" (p. 100). Notwithstanding Mill's attraction to the abstract concept of a *pouvoir spirituel*, his plans for institutionalizing it were from the beginning hedged about with Benthamite qualifications—precisely to assure that it would possess only moral, and not coercive, power. Even *The Spirit of the Age*, the most Saint-Simonian of all Mill's works, was by no means an unqualified paean to authority (Robson admits that his quotations are taken out of context); and in the autumn of 1831 Mill wrote to John Sterling that "in the present age of transition, everything must be subordinate to *freedom of inquiry*."

Confusion also mars Robson's account of Mill's ethical theory. The chapter entitled "Moralist" is weakened throughout by Robson's failure to take account of chronological changes in Mill's ethics. Though ostensibly discussing the period after 1843, he frequently quotes articles written in the 1830s without realizing that they contain confusions which Mill later attempted to clear up and concessions he was later un-

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willing to make. Moreover, in discussing Mill's view of the moral value of art and religion, Robson confuses the motive with the moral criterion of an action (p. 122), a distinction basic to the ethics of the utilitarians. He is also unclear about what he calls "ends" in moral questions. His use of the word itself is ambiguous: he never specifies whether "end" refers to a standard of morality or to goals which individuals actually pursue. The confusion becomes apparent when he states that Mill allows "various ends proposed by other systems to have a place" but "denies to them only the first place" (p. 123). The source which Robson cites as evidence leaves no doubt that for Mill utility is the only *criterion* of actions. The distinction Mill drew was between acts which should be legally compulsory and those in which individuals should be left free. What he asserted was that in the latter sphere individuals would in fact pursue many goals—all of which should be judged by the single standard of utility.

Furthermore, Robson wrongly states that Mill gave preeminence to individual over institutional reform. The dichotomy is fictitious, for many passages which Robson himself quotes exemplify Mill's conviction that evil institutions impede the improvement of individuals. Indeed, immediately before asserting the preeminence of the individual in Mill's thought, Robson notes Mill's belief that when "institutions are actively preventing moral and intellectual improvement," the reformer must "strive for beneficial alterations" (p. 126). In addition, by repeatedly asserting that Mill's "most important modification of utilitarian ethics" was his "concentration on the individual," Robson misstates both Mill's contribution and the position of his predecessors. Bentham and John Mill were equally concerned with individuals (for Bentham, the supreme task of the legislator was to insure social harmony by influencing the conduct of the *individuals* who composed society). The difference was that Bentham concentrated primarily on individuals' *actions*, whereas Mill was concerned as well with their thoughts, emotions, and desires.

To compound his errors, Robson contradicts himself on the role of the intellect in social progress (pp. 125-126, 152-153). He is mistaken in thinking that the chapter on "Circumstances Influencing Sensibility" in Bentham's *Principles of Morals and Legislation* is concerned with qualitative differences in pleasure; hence his statement that Bentham "analyzes [qualitative differences] away in terms of bodily organization" has no bearing on the issue under discussion. And he obscures the views of Bentham, James Mill, and John Mill on the relation between selfish

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and sympathetic feelings. In John Mill's view, according to Robson, "man has sympathetic as well as selfish feelings, and morality depends upon the former, not as Bentham and James Mill thought, upon the latter" (p. 135). The statement is both ambiguous (in what sense does morality" depend" for the utilitarians upon feelings of any kind?) and wrong (if it is meant to imply that Bentham and James Mill attributed to man only selfish feelings). Indeed, the meaning of "unselfish" is one of the most complicated and basic problems faced by John Mill. Robson has contributed little to its elucidation.

The cumulative effect of the failings discussed above is to vitiate Robson's overall analysis of Mill's thought. His confusions about Mill's chronological development and intellectual debts lead him on some issues (such as the *pouvoir spirituel*) to exaggerate Mill's shifts and on others (such as ethics) to overemphasize his consistency. The latter tendency, in turn, leads Robson to impose the wrong kind of unity on Mill's thought. The unity of Mill's philosophy derives not from a settled body of doctrine but from constant goals and a series of recurring problems. Within this framework Mill never ceased to modify both the doctrines and the emphases of his thought. Significantly, the best sections in *The Improvement of Mankind*—such as that on Mill's attitude toward socialism—are precisely those in which Robson is most aware of Mill's shifts and turns. The most unsatisfactory—such as the chapter on ethics—are those in which he pays them least attention. Most serious of all, even within his own structure, Robson's attempt to unify Mill's thought is largely superficial and mechanical. He frequently reminds us that utility was Mill's ultimate standard. Yet philosophic and stylistic ambiguities hopelessly obscure Robson's treatment both of Mill's problems and of his attempts to solve them. As a result, *The Improvement of Mankind* provides neither a satisfactory exposition nor a conceptually rigorous analysis of Mill's thought.

WENDELL ROBERT CARR
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The Correspondence of Jeremy Bentham, Vol. I: 1752-76; Vol. II: 1770-80 *edited by Timothy L. S. Sprigge. London, Athlone Press, University of London, 1968.—xli, 383 pp.; xiv, 542 pp. £8, 8s.*

"The whole world steals from him," said Tallyrand of Bentham, "and still he is always rich!" It may once have been so, but for over a cen-

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tury his books have been largely unavailable, and he has long since sunk into a cliché figure of denigration in political theory texts. Here, then, is an occasion for great rejoicing, an event of the first importance in publishing history: at last, after some two centuries, a complete edition of Bentham's works. Hitherto, there has been nothing but Bowring's "complete" edition of 1843, long since out of print and thoroughly bowdlerized, printed in tiny type on awkward double-columned pages, beset by Victorian pieties and therefore ignoring all of Bentham's many writings on religion and sex, and altogether a hasty, slovenly jumble. At last it will be possible to see Bentham large and whole; and it may well be that a new generation of hitherto thwarted, eager students will rediscover this great man.

Under review are the first two volumes of an eventual thirty-eight, that promise, in the words of its highly competent General Editor, Professor J. H. Burns, to be "comprehensive in scope as well as definitive in text." They have been skillfully edited by Timothy L. S. Sprigge of the University of Sussex; and the whole enterprise is under the general supervision of the Bentham Committee with indispensable financial help from the Rockefeller Foundation, the Pilgrim Trust, and the British Academy. These two volumes deal with Bentham's early correspondence and cover the years 1752, when he was four, to 1780, when he was thirty-two and in the midst of writing his masterpiece, *An Introduction to the Principles of Morals and Legislation*. Unfortunately the soaring, exciting promise of the edition as a whole is somewhat earthbound in these two rather pedestrian, leaden-footed first volumes. What we should hope to find would be a mass of insights into the origin and development of Bentham's ideas, but regrettably there is almost nothing of the kind here. We know very well that he picked his friends' brains bare, as they picked his, in all-night talk-fests, but he saw such confidential intimates as George Wilson, John Lind, and his younger brother, Sam, so often that he had no need to confide on paper. These letters are largely social chit-chat and rather trivial business details about sending dirty laundry or tailors' bills. They are perhaps of more value to social and administrative historians than to theorists: the Benthams were a very close family of bourgeois *rentiers*, and the letters give a full picture of their kind of obscure, busy life in eighteenth century London. There is a rather amusing hunt for a monied wife for Bentham.

But perhaps the most interesting theme of all is his preoccupation with Sam's training as a naval engineer, a profession then unknown in England which the family had more or less to carve out *de novo* for the boy. Rivalry, nepotism, ignorance: all these were such obstacles that

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finally Sam had to leave England to make his career in Russia as the manager of Prince Potemkin's estates, where Jeremy later joined him for a two-year visit. All of this was bitter grist to Bentham's mill and will be reflected in larger purposes in the many volumes to come.

The current working plan for the thirty-eight volume series calls for ten divisions: correspondence, principles of legislation, penology and criminal law, civil law, constitutional law, political writings, judicial procedure, economics and society, philosophy and education, and religion and the church. For the venture as a whole, there can be nothing but praise and gratitude. A highly skilled corps of professionals is hard at work doing a vital job.

MARY PETER MACK
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Non-Violence and Aggression: A Study of Gandhi's Moral Equivalent of War *by H. J. N. Horsburgh. London, Oxford University Press, 1968.—ix, 207 pp. 35s. $5.60.*

William James's phrase "moral equivalent of war" is quite misleading in this work as in most other contexts in the political literature. Mr. Horsburgh, like most writers, takes the moral monstrosity of modern warfare for granted; what he seeks is a practical equivalent which can achieve the same objectives without the same moral and other costs. Unlike most of us, he is admirably explicit and systematic in stating his assumptions and hypotheses concerning the likely costs of war and of possible alternative measures of defense, as well as the values that all defense efforts must seek to protect and advance.

This study is by no means an uncritical exegesis on Gandhi's teachings. Horsburgh's concern may partly be to propagate the Gandhian gospels, but he is clearly out to reach the critical reader, including the intelligent military analyst, with a hardnosed practical case for gradually increasing the role of nonviolence in national defense planning. He wants to convince rational men of the utility of *satyagraha* as a source of strategic as well as tactical guidance in defending a way of life, especially a democratic way of life—indeed, of the indispensability of *satyagraha* if our western, relatively free institutions are to survive in our modern world. By "rational men" he means those who are able to transcend the conventional prejudgments about the indispensability of military preparedness and also to dispense with the usual pacifist illusions.

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On this last subject, unlike many pacifists Horsburgh accepts the premise that in the foreseeable future military violence will remain "the ultimate arbiter in human affairs"; at least in the West, most decision-makers will continue to stick to this assumption and hardly bother to examine it. Moreover, he is sharply at odds with nuclear pacifists, charging that "the position of those who refuse to renounce war but who propose to deny themselves the possession of the most potent weapons seems to be fatally confused," since any military war with an enemy without the corresponding scruples is bound to lead to defeat; his hypothesis is that whichever side is likely to lose a given war is likely to shed whatever scruples it may have had initially about using the most destructive weapons available.

Most simply described, the study inquires whether a Gandhi-type nonviolent struggle can serve as a practical equivalent of war by satisfying these criteria: that it is a method of resistance distinct from war; that it suffers less (or not at all) from the moral deficiencies of war; that it can be used in all situations in which war has been thought necessary or permissible; and that it offers some prospect of being effective (this last criterion is subdivided and made more precise in chapter 6).

The bulk of the first half of the book is given to a cogent critical examination in brief of the theory and some of the past practices of *satyagraha*. But the importance of the book is in its second half, beginning with chapter 4, entitled "Defence: Some Central Issues." With real sophistication Horsburgh spells out a variety of categories of moral and empirical issues which are usually tangled together, it seems, in war books and peace books alike. One may quarrel with some of the author's flat assumptions, for example, on exactly what is to be defended, but it is a rather unusual privilege in the literature on strategies of war and peace to have the option of quarreling with, or agreeing with, such precise statements as are offered here. Admirable, too, is Horsburgh's continuing attention to and competent assessments of many probable psychological processes at work in defense planning as well as in warfare and nonviolent struggles.

The author's knowledge of the American controversies over nonviolence appears rather limited, and his empirical judgments on the merits of violence and nonviolence in attempts to resolve racial or class struggles often seem questionable. Hardly any attention is paid to the crippling impact of institutionalized violence of colonial, racial, or class oppression; *violence* is never defined, and as usual this omission means that the term is associated with ruthless efforts to bring about changes and not with ruthless efforts to preserve, excepting outright colonialist oppression in India and South African Apartheid. Other objections

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could be listed, but they are minor. On balance this is a book to be recommended to all who are concerned with and who are open to provocative, lucid, systematic argument about the most urgent issues of our time.

CHRISTIAN BAY
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Origins of the Fifth Amendment: The Right Against Self-Incrimination *by Leonard W. Levy. New York, Oxford University Press, 1968.—xii, 561 pp. $12.50.*

This is a work of monumental scholarship—broad in scope, thorough, carefully annotated, accurate, and imaginative—a history of the background of the right against self-incrimination from its beginnings to the ratification of the Fifth Amendment in 1791. It is difficult to understand how Professor Levy, with his academic and administrative duties, to say nothing of his other recent writings, could find the time to carry out the research and related tasks that lie behind the writing of this book. It is no exaggeration to say that is one of the most important of recent studies on American constitutional origins. More than half the book is devoted to English antecedents because the origins of the right are essentially English and are related to the development of the accusatory system of criminal justice and of fair trial. The main text is followed by an absorbingly interesting Appendix on Talmudic law, which recognized the right in ancient Jewish law, but the conclusion is reached that that law afforded no precedent for the common law right. There are extensive, detailed notes, supplemented by a long bibliography and an index.

Prefacing the full discussion of the development of the right is a comparison of the inquisitorial system of continental Europe with the accusatorial system of England founded basically on jury indictments. The former system presumed the guilt of the accused on the basis of secret interrogatories, whereas the latter was a public prosecution by the crown's attorney. The main discussion begins with tracing the introduction, growth, and enforcement of the oath *ex officio* and with Elizabeth's persecution of Catholics. None of the latter, for example Campion and Leigh, invoked the right of self-incrimination because no such "right" was then recognized in any of the courts. Fully detailed

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chapters follow on the cases of Whitgift, Cartwright, and others and careful consideration is given to the inquisitorial character of procedures before the High Comission. The flood of writs of prohibition which followed the accession of James I, and frequent protests in parliament against the *ex officio* oath, were abated between 1616 and 1633, and even the Petition of Right did not speak against it. Two of the most interesting chapters in this portion of the book deal with John Lilburne's trial and the eventual abolition of the oath in civil and criminal proceedings. When finally recognized in England, the right was, in Levy's words, "one of the ways of fairly determining guilt or innocence. . . . The right implied a humane or ethical standard in judging a person accused of crime, regardless how heinous the crime or strong the evidence of his guilt or innocence" (p. 332). Closely linked to freedom of speech and religion, the right became a part of the common law heritage which the American colonists acquired from England.

Despite the fundamental importance of the English background of the Fifth Amendment, American historians and political scientists will be chiefly interested in the last three chapters of the book dealing with the recognition and establishment of the right in America. The first of these describes the situation in the colonies in the seventeenth century. When and how the right became the law of a particular colony has proven to be an unanswerable question, even to Levy. However, on the basis of available evidence, for instance the Wheelwright, Hutchinson, and Child trials in Massachusetts Bay, the author concludes that the right was recognized by the end of the century, albeit shakily and unevenly and accompanied by numerous breaches and inconsistencies. During the eighteenth century, however, the right received gradual recognition in the colonies with the increasing reception of the English common law, particularly in the criminal procedure field, so that by 1776, "colonies and mother country differed little, if at all, on the right against self-incrimination" (p. 404).

The background of the United States Bill of Rights is the subject of Levy's final chapter. That background is far better known than is the material discussed in the first 400 pages. In 1776, several states included the right in their constitutions, but these early bills of right tended to be imitative and irrationally selective. Nevertheless, nearly all the state conventions recommended amendments to the Constitution, but apparently only four recommended the inclusion of the right against self-incrimination. In the first Congress, however, James Madison proposed an amendment which was to be substantially the basis of the Fifth Amendment. It

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seems clear from Levy's evidence that the self-incrimination clause, when ratified, was intended to protect not only the criminally accused but witnesses as well and did not abridge the common law right (p. 429, n. 35).

The concluding sentences of the book reflect Levy's judgment that the framers viewed the right as a central feature of the accusatory system of criminal justice and, above all, that in a free society the determination of guilt or innocence through procedures in which the accused did not contribute unwillingly to his conviction was more important than punishing the guilty.

GEORGE L. HASKINS
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The Jurisprudence of John Marshall *by Robert Kenneth Faulkner. Princeton, Princeton University Press, 1968.—xxi, 307 pp. $10.00.*

The title of this book leads one to expect a systematic exposition of Marshall's legal philosophy based upon an analysis of his case law. His circuit court opinions fill two volumes, and he wrote 519 Supreme Court opinions in many fields of law. Mr. Faulkner, however, is curiously uninterested in law, legal history, or legal philosophy. He discusses only sixteen cases, some slightingly, and cites only about twice as many more in his footnotes. By "jurisprudence" he means political thought or "Constitutional understanding." He has written an excellent but highly formalistic analysis of Marshall's constitutional ideas, arranged mainly around such categories as liberty, interest, sovereignty, republic, federalism, and judicial function. The results tends to be arid and abstract. In contrast to his slight use of even the judicial opinions in constitutional law, he makes heavy and illuminating use of Marshall's *Life of George Washington* and of a small portion of his correspondence. There are many discursive though interesting footnotes, but neither a table of cases nor a bibliography.

Excepting the two long appendixes—one a brilliant and controversial contrast between Holmes and Marshall, the other a first-rate defense of Marshall's opinions in the Burr Treason Trial—there is little in the book that is fresh in interpretation or information. To be sure, Mr. Faulkner strives for novelty yet overstates his position or lets his evidence con-

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tradict him. He claims, for example, that "the encouragement of human liberty formed the great theme and purpose" of Marshall's life. The principal evidence is his tight definition of treason and his opposition to the Sedition Act. But that opposition, it turns out, was purely expediential; Marshall believed that the government, despite the First Amendment, could suppress seditious or licentious speech. Mr. Faulkner also acknowledges, as we have always known, that protection of property was foremost in Marshall's encouragement of human liberty. The claim, however, is that his fundamental concern was for "the industrious acquisition of more possessions, not possessions as such," but the case-law hardly bears out the proposition. Elsewhere Mr. Faulkner concedes that Marshall's fundamental concern was for "vested rights," although he alleges that the "great" opinions "did not fundamentally involve 'property' as such." That allegation eliminates from the category of greatness the celebrated commerce and contract clause opinions. Another of Mr. Faulkner's propositions is that Marshall's nationalism has been exaggerated. I think that only W. W. Crosskey is guilty on this count, yet Mr. Faulkner's own discussion proves that it is almost impossible, except for Crosskey, to exaggerate Marshall's nationalism.

I found particularly irritating the author's incessant and often unexplained use of the adjective "Lockean." Thus Marshall is presented not only as a "liberal Republican," which any Jeffersonian would deny; he is also the pre-eminent Lockean jurist. Lockean modifies such words as capitalism, constitution, enlightenment, equality, individualism, jurisprudence, liberalism, order, populace, persuasion, republicanism, scrolls, among others. Accordingly the reader confronts such statements which I think meaningless or pretentious (though others may find them profound) as "For a Lockean order, Lockean men must rule" and Marshall approved of self-government as long as all citizens would model their conduct "on the Lockean individual."

I could quibble with a number of points in the book (e.g., Madison, not Marshall, as alleged, was "the last of the original Federalists"), but they probably deserve to go unnoticed. It is no quibble, though, to repudiate as nonsense the proposition that Holmes believed there were "no fundamental constitutional principles" nor to expose the error in the discussion of Marshall's "well known letter" of 1801. Dividing the opposition party into "absolute terrorists" and "speculative theorists," Marshall classified Jefferson with the terrorists. Mr. Faulkner, always ready to show Marshall's moderateness, statesmanship, and judicial temperament, says he "sensibly" put Jefferson in the theorist class.

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Beveridge in his *Life of John Marshall* (III, 11) quotes the entire passage; if he is correct, Mr. Faulkner, who paraphrases and quotes snippets, is wrong. Nor is it wholly true to assert that Marshall had long held the views on judicial review expressed in the Marbury case. In *Ware v. Hyton* (3 Dallas 199, 211 [1796]) he argued that "the judicial authority can have no right to question the validity of a law"; moreover, in a letter of January 23, 1804, he advocated a "reversal of those legal opinions deemed unsound by the legislature . . ." (Beveridge, III, 177). Errors aside, Mr. Faulkner possesses a fine analytical mind, and his subsequent books will probably be better than this one, the outgrowth of a doctoral dissertation in political science at the University of Chicago.

LEONARD W. LEVY
Brandeis University

A Constitutional Faith *by Hugo LaFayette Black. New York, Alfred A. Knopf, Inc., 1968.—xvii, 66, viii pp. $3.95.*

A good part of Justice Black's magic has always been his brevity and simplicity; Justice Douglas may be the fastest man on the Court, but Black can be the tersest. In these Carpentier Lectures at Columbia University, Black says his say about constitutional interpretation in general, due process, and free speech in 66 pages and 18,000 words. The whole volume is about the length of a long case, such as the opinion in *Miranda v. Arizona*, for example.

The practical consequence, since Black has been a dominant figure of our constitutional age, is that it will be a rare course in constitutional law which fails to suggest reading of this volume; it pulls together more, more tightly, than any other writing in the library by and about Black. Its total lucidity and verbal simplicity is another hallmark of its author; Black may provoke a reader, but he will rarely confuse him. Partly this results from another Black trademark, simple word choice. I choose page 21 at random. There are only four words on the page with as many as four syllables, and they are *Constitution, legitimate, repetition, recognition*. He wants to be understood by someone who stands halfway on the educational level between a Clay County farmer and a Harvard Law School graduate.

With the recent retirement of Senator Carl Hayden of Arizona (who went to the Senate with Black in 1927), Black is the unquestioned senior,

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not only of the Supreme Court, but of all of today's American life. He is our bridge to the New Deal, to the depression of the 1930s, to the uneasy prosperity of the 1920s. But in a larger sense, he is our bridge to the seventeenth and eighteenth centuries. For no other public figure is the step so sure, so easy, and so unassuming to Lilburne, to Wilkes, to a seventeenth century preacher named Udall, to Mr. Madison; and all are here. In Black's mind, the plane of vision is larger than today. He absolutely rejects the concept that because today's government may not abuse, tomorrow's will not. As he sees it, there is as much need now to build barriers against the return of the Star Chamber as ever there was. He rejects utterly the complacence, that all is well as long as this Court sits. It may not sit. As he says with the force of personal reminiscence of one who was there in 1936 and 1937, "To the people who have such faith in our nine Justices, I say that I have known a different Court from the one today" (p. 11).

This leads Black to the theme of each lecture: the crucial factors in interpreting the Constitution are "language and history," not "reasonableness or desirability" (Lecture I). Substantive due process should not be revived in any form, under any other clause; the worst opinion of the Court of recent years is *Rochin v. California* (the stomach pump case), because the "shock the conscience" test gives judges a tyrannical discretion they should not have. Black's conscience is shocked by the Connecticut birth control legislation, but that did not, in his mind, make it unconstitutional (Lecture II). Free speech and press must be absolute; he rejects the Meiklejohn refinement of limitation to political speech, and so encompasses libel and obscenity as protected. But speech only, not action. The community has no obligation to open its buildings or public places to demonstrators, so long as controls are evenly administered; government by a shrieking mob is wholly incompatible with government by a legislature freely elected under First Amendment standards, to which Black is committed (Lecture III).

This synopsis is too much of a bouillon cube, over-condensed. There is needed talk about "activism" and "self-restraint," for example; but the sketch gives an idea of what's here.

Black is sensitive to the recurrent theme that he has changed with the passing years; he alludes to the suggestion early (pp. xiv, xv), and is obviously answering it in the last lecture. As a "Black author," I am asked more than any other question whether the Justice has "changed." The answer, as this volume demonstrates, is that he has not changed significantly with the years. (We are speaking here of major change. As I wrote in 1949, "Black frequently changes his mind when convinced he is wrong. He did so in the Senate and has done so on the Court [J. Frank,

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Mr. Justice Black, New York, 1949, p. 134].) But times have changed, and he may not have changed with them. The real change is the change in Black's relation to his constitutional era, and here he may well be out of joint with shifting cycles, in the same sense that a republican of Weimar was out of joint with the Third Reich.

Black's constitutional views do accord with Lilburne's, or Udall's, or Wilkes's, or Madison's. They are absolutely out of accord with the overwhelming congressional vote which approved Title II of the Safe Streets Act in 1968, or with a mob seizing a university campus or building, or with the proposal apparently about to spring from the new administration of "preventive custody" in the District of Columbia, or with proposals to resegregate universities from a black point of view. To the extent that HLB is in conflict with dominant views, it is not the first time. He moved from Court critic in 1936 to leader in the 1940s back to helpless dissenter with Vinson and spokesman with Warren. His views on criminal justice have largely prevailed and may again unless the new administration gets its hands on the Court before "Safe Streets" arrives, and the country is rallying to his views on disorders. This, in short, is a "constitutional faith" with impact, and the professions of law and political science will do very well to take very close account of it.

JOHN P. FRANK
Phoenix, Arizona

Money and American Society, 1865-1880 *by Walter T. K. Nugent. New York, The Free Press, 1968.—xv, 336 pp. $7.95.*

The title gives only a vague clue to this book's content. Nugent asserts at the outset that his subject is "the response of groups in American society to changing social conditions in the years immediately following the Civil War." He used the "money question"—the interlocked ongoing controversies over specie resumption, silver coinage, the greenbacks, and the funding of the national debt—as the key to understanding a perceived transformation in group attitudes, rhetoric, and alliance. Nugent returns to this broad theme time and again, yet the bulk of his study is more a detailed narrative of the political struggles that arose over establishing monetary standards, and particularly the maneuvering behind the scenes, than a rigorous analysis of interest groups, their rhetoric, or their ideologies. His major theme is that before 1873 there was a broad,

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general consensus, though admittedly a precarious one, among the major political interest groups in their view of what American society should be like, in the common assumptions of their rhetoric, and in their concepts of public morality. After 1873, the "money question" as the main unsolved and unavoidable public issue functioned as a rock that split open the cracks in that consensus. The temporary settlement and abatement of the issue by 1880 helped recapture the consensus, but achievement of the settlement had been obtained only through new and divisive alignments that polarized opposing views and assumptions among interest groups. The divisions remained unhealed for the rest of the century; the resulting group antagonisms set the basic and essentially unchanged pattern for the Populist interlude and for political struggles in our own time.

Nugent's generalizations rest on abundant research in newspapers, journals, and that long neglected source for interest-group opinion, the incoming correspondence of congressional committees. Such evidence needs careful and rigorous handling; it does not seem to offer sufficient support for the rather sweeping conclusions, interesting and suggestive as they may be.

On the level of the narrative itself, there is no doubt that Nugent has made a solid and fresh contribution to historical knowledge. As he acknowledges, much of the study overlaps and cuts across recent thorough and painstaking work by Unger, Sharkey, Weinstein, and others. It confirms and gives additional support to the revisionist interpretations of the Greenback movement and the "Crime of '73," bringing new detail and original insights to the analysis of those events. His scholarship is generally sound, his prose is vigorous, and he clarifies the interrelationship between the various aspects of the money question in a convincing synthesis. Most important, he keeps the subject more firmly in its international context than any of his predecessors, combining his American materials with original work in European archives.

Though Nugent's treatment of details is on the whole masterful, it is sometimes hurried, and several assertions seem open to question. For example, he ignores the critical differences between the interests of importers and exporters, lumping them together as "foreign traders." His claim that economic interest and ideology matched on the money question is belied by the consideration that many manufacturers and other businessmen who joined the ranks of the "sound money" phalanx were acting against their own short-run interests. On occasion, the treatment of broad economic developments becomes simplistic. These are relatively minor deficiencies, however. The book is a welcome and useful addition

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to the cumulative scholarship that is re-shaping our understanding of political and economic developments in the post-Civil War period.

MORTON ROTHSTEIN
University of Wisconsin, Madison

The Private City: Philadelphia in Three Periods of its Growth *by Sam Bass Warner, Jr. Philadelphia, University of Pennsylvania Press, 1968.—xii, 236 pp. $5.95.*

One of the most pressing needs of urban history is a conceptual framework which can order the structure and processes of urban development over long periods of time. The traditional "urban biography" has long since been under fire. But what shall replace it? Sam Bass Warner, Jr.'s *The Private City*, an account of the development of Philadelphia from 1770 to 1930, is implicitly an attempt to provide an alternative.

Most useful, undoubtedly, is his method. Rather than developing a narrative, year-by-year or decade-by-decade account of the history of Philadelphia, Warner undertakes a cross-sectional approach. He describes intensively three different time periods, the decades of 1770-80, 1830-60, and 1920-30. Within each of these periods, Warner describes a variety of social, economic, and political factors, but not always the same ones in the different decades. The most consistent attention is given to the spatial distribution within the city of its economic activities and its population. For some decades he develops information about leaders whom he considers to be representative or about ethnic tension or industrial growth. Some of these chapters are quite successful. His account of the ethnic and religious riots of the 1830s and 1840s, for example, is more complete than any available elsewhere and provides a significant context for the broader development of ethno-cultural differences in mid-nineteeth century American politics.

Far less successful, however, is the overall framework, summarized in the title. Ostensibly, Warner is concerned with the "private" city. He argues that decisions about urban development throughout Philadelphia's history have been made by private individuals and groups rather than by public instrumentalities. Throughout the book he focuses on the "enduring tradition of privatism in a changing world," the way in which past private decisions influence heavily the response to contemporary social change. It is significant that he defines the nature of "pri-

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vate" forces not as liberal historians have done, namely as large corporate institutions and machine politicians, but as an accumulation of innumerable decisions made by innumerable numbers of people in their private lives. This is a welcome relief from earlier notions about the dynamics of urban politics.

But more important is the inadequacy of the concept of "privatism" in the first place. The fact of the overwhelming weight of private decisions in urban development seems clear enough. But how relevant is this in the formulation of a conceptual framework of urban development? How much does the theory generate the conceptualization of subproblems, the design of case studies to illuminate points of interpretation, the articulation of distinctions and relationships? To this reviewer the concept seems to have little such potential. All, in fact, that Warner can do is to reiterate the point that private decisions dominated urban development. Especially missing is a focus on networks of human relationships, the patterns of human interaction which generate structures, the effort to locate tendencies toward change and lack of change in specific sectors of structure, the elaboration of the vertical dimension of human interaction. The importance of a focus on private action within the city seems to pale into insignificance in the face of these larger aspects of human structure.

One suspects, however, that Warner's framework is far more normative than descriptive. He seems to be most interested in why urban "problems" haven't been "solved," why public policies which he feels to be important have not been tackled. "Why this lack of progress?" he asks, and the answer lies in the "habits of community life" which lie deep in the historical process of urbanization. The rationale of the entire book, therefore, is an explanation for the failure of cities to develop in a particular way, according to Warner's norms, rather than a systematic description of the evolution of cities themselves.

One finds in this book, surprisingly, an historian who is still closely tied to the normative values of "progressive" history, still focusing more on explaining why desired changes—desired in "progressive" terms—were not brought about rather than on the process by which social change took place. The book provides another link in the argument of those who seek to counteract, to remain resigned about, or to express nostalgia over the failure of rationality and system in modern urban life, but it remains limited in its capacity to generate long-run contexts for understanding urban history.

SAMUEL P. HAYS
University of Pittsburgh

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California's Prodigal Sons: Hiram Johnson and the Progressives, 1911-1917 *by Spencer C. Olin, Jr. Berkeley and Los Angeles, University of California Press, 1968.—ix, 253 pp. $6.95.*

George Mowry's splendid work, *The California Progressives* (1951) did not appear to leave room for another book on the subject, but Olin discerned an opportunity. An extensive secondary literature had built up, the Hiram Johnson manuscripts had become available, and perhaps Mowry stood in need of revision. Beyond that, there was no published biography of Johnson nor a study of his administrations as governor. While *California's Prodigal Sons* is neither of these, it focuses closely on Hiram Johnson from 1911-1917, using the advantages of new materials available since 1951, and tells again the story of state-level reform in California.

While much of the story is necessarily familiar, Olin is able to emphasize what had been insufficiently appreciated earlier, the extent of the progressive accomplishment in the areas of labor legislation (largely forced on Johnson by organized labor and advanced reformers like Simon Lubin and Katherine Philips Edson) and scientific management. These had previously been somewhat obscured by the Direct Democracy measures of the progressives. Hiram Johnson himself emerges as the man we thought him to be, only more so—suspicious, pugnacious, self-centered, individualistic ("I never did submit to discipline") yet so thoroughly attuned to the psychological currents of his time as to be virtually unbeatable.

Olin is a capable writer of traditional political history and gives close attention to the controversies that have engaged historians, such as who was to blame for the Virginia Hotel snub (of Johnson by Hughes, who then lost Califronia by 4,000 votes) and whether Johnson supported Hughes loyally in the campaign. Political scientists will be glad to know that progressivism has attracted scholars generating more meaningful controversies—men like Samuel Hays and Robert Wiebe, who are asking questions about the social structure of the movement. The important questions now about progressivism are its class base, and especially its relation to entrepreneurial groups, labor, and new professional elites. Olin is well aware of these questions, and attempts to shed light on them whenever the opportunity arises. That his book does not significantly advance our systematic (as distinct from our narrative) understanding of progressivism is partially due to Olin's casual interest in such questions, which leads him to occasionally fail to exploit what appears to be vitally important evidence, such as the entrepreneurial sources of the

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revolt against the Southern Pacific. But primarily it is due to the lack of a common and strict terminology and methodology among historians, which restricts our efforts at cumulative scholarship.

Olin's research is thorough, so much so that the footnotes (at the back) are often more absorbing to the scholar than the text. The style is clear and entirely free from barbarisms and strain. The book has unsurpassed contemporary relevance for a monograph, for it deals with a grassroots, "spontaneous," and "idealistic" effort by amateurs to take over a major party, and the progressives manage to resemble both the followers of Barry Goldwater and Eugene McCarthy. California progressivism can certainly stand such intensive study, for here progressivism combined all its facets—Direct Democracy, labor legislation, regulation of industry and transportation, conservation, agrarian demands, and enforced morality. The traditional sources and approaches have now probably yielded their all, but this latest product, a clear and meticulous chronological narrative, will be indispensable to scholars of a more analytical turn of mind.

OTIS L. GRAHAM, JR.
University of California, Santa Barbara

Republican Foreign Policy, 1921-1933 *by L. Ethan Ellis. New Brunswick, Rutgers University Press, 1968.—xii, 404 pp. $10.00.*

This sixth book by a distinguished scholar of American foreign relations is a synthesis of the printed literature of its subject combined with close knowledge of manuscript sources for the era 1925-1929, for which era the author already has published a careful study. Ellis seeks to examine American foreign policy not as it might have been, but as an individual of the time would have viewed it. To this effort he brings the special qualification of having lived through his period as an adult American. Now retired from the history faculty at Rutgers, he was a teacher and student during the 1920s.

The book sets out the temper of the time in an enlightening chapter entitled "Signs and Portents" and follows with another entitled piquantly "O Tempora, O Homines." There then is examination of the leading issues in foreign policy during the twelve-year Republican Restoration. The author makes a distinct effort here to get under the surface of events

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and does so admirably. His scholarship lies everywhere at hand, for Ellis knows his subject so well that he can set out complexities in a few sentences. One might say that Ellis is not a beginning lecturer, giving all the wearisome details of a problem in hope of filling out an hour's lecture with an hour and one-half preparation; in his studies he has gone into his topics in so much of their detail that he now can shorten his explanations to the points which count. His explanation of naval disarmament, a problem which often takes the uninitiated book-writer into bewildering detail, comes clearly to the attention of the reader, as does likewise the explanation of the Kellogg-Briand Pact and the various trials of Latin American diplomacy of the time. The reviewer can only look forward to this book's early appearance in paperback so that it can reach a large university audience.

There is one point on which, at least, a disagreement with the author is possible, and this point ought to be set out, even though Ellis doubtless can defend his position. The author believes, with the historian William A. Williams (who some years ago published a much-noted article on the subject), that the 1920s was not an era of isolation, that indeed the period marked a large American participation in foreign affairs. Now, this was not necessarily so. It is true that the United States took large interest in foreign affairs during that time, but it was interest and little more. The American government did not participate in the balance of power in Europe. Standing apart from that balance, refusing piously to use either economic or military sanctions in support of peace, the United States does, in retrospect, appear to have adopted an isolationist policy. Perhaps it is this retrospect of our own power-crazed time, though, which the author of the book under review is seeking to avoid.

ROBERT H. FERRELL
Indiana University

American Diplomacy and the Spanish Civil War *by Richard P. Traina. Bloomington and London, Indiana University Press, 1968 —xi, 301 pp. $10.95.*

Soundly researched works on the 1930s have recently multiplied apace. This is only natural for, as William Miller has noted, "the historian steps in after a movement or a process peters out." Thus Frederick J. Turner focused our attention on western history just after the frontier had passed, and Marcus L. Hansen analyzed the immigrant factor in American history hard upon the closing of our gates to the hordes of Europe.

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The war in Vietnam has highlighted the weaknesses of a globalism set in motion in the latter part of the F.D.R. era, while recent political convulsions have seemingly outdated both the Roosevelt electoral strategy and the domestic liberalism which rested upon the success of that strategy. Professor Traina's excellent study, based upon manuscript sources found in some eight repositories, ranks among the best books produced by a young scholar on the diplomacy of the Axis years. The author has mastered his sources, his arguments are penetrating and telling, and his book is studded with delightful pen portraits of the leading characters in his story.

Some historians use the twin benefits of a lengthened historical perspective and newly available evidence to indict the policy-makers of yesteryear for making the wrong decisions. While Professor Traina finds many justifiable faults with the diplomacy of Hull's State Department, he left this reviewer with the impression that under the existing circumstances few options remained open to the New Deal diplomats. They might well, Traina infers, have followed international law and precedent from the very beginning. This course would have allowed them to shore up the legitimate government of Spain at Madrid, without a charge of favoritism. However, once they bypassed this splendid opportunity there was no other course to follow than the one they did even if it made a Franco triumph inevitable.

There is little mystery behind the decision of Washington to follow the lead of London and Paris in the futile attempt to curb foreign intervention in the Spanish Civil War. The outbreak of the revolt came at a time when the policy of American isolationism stood almost unchallenged, and Roosevelt could hardly have been expected to have defied this tradition in an election year. Moreover, during the initial stage of the conflict, the president allowed Hull to formulate policy. As the situation deteriorated, Roosevelt paid increasing attention to it, but he soon found that he had been "legally immobilized" by the act of January 7, 1937 which extended the existing arms embargo to both factions in Spain. While Traina condemns F.D.R.'s irresolution during the Spanish crisis, in contrast to many previous writers he pins the blame for this shackling embargo on Assistant Secretary of State R. Walton Moore rather than on the president.

Many contemporaries realized that a Franco victory would prove a great boon to the Axis. Inasmuch as human beings are not clairvoyant, none of them could foresee that in the long pull the United States had less to fear from a rightist than from a leftist triumph on the Iberian peninsula. Secretary Hull, for instance, who did so much to keep the president from reversing the policy of non-intervention, was never convinced that a Loyalist victory would be in our own interests. Ambassador Claude G.

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Bowers whose facile pen later distorted the historical picture, was by no means consistent during the actual crisis in defending the Loyalists to his superiors in Washington. Roosevelt wavered, and late in the conflict he might have lifted the embargo had he believed that he had the legal power to do so under the terms of the May 1, 1937 Neutrality Act. The trouble was, however, that the pro-Loyalist wave in the United States did not crest until the fighting in Spain was all but over. By this time Roosevelt was faced with a Far Eastern crisis, the aftermath of Munich in Europe, the political comeback of the Republicans, and the necessity of convincing the isolationist phalanx on Capital Hill that the White House's preparedness program was aimed at defense only. To have seized the initiative in Spain so late in the war would have invited great domestic risk without any certainty at all of halting Franco. The author of this sprightly book makes it clear that such an intervention would have been most foolhardy. During the Spanish war F.D.R was still in the chrysalis period of his statesmanship but he was far too sensible to have divided the American people in behalf of a cause no longer able to be won.

SELIG ADLER
State University of New York at Buffalo

The Politics of War: The World and United States Foreign Policy, 1943-1945 *by Gabriel Kolko. New York, Random House, 1968. —x, 685 pp. $12.95.*

The Second World War, according to Gabriel Kolko in this massive book, toppled the Old Order in Europe and Asia, thus completing a process under way since 1914, and set the stage for an incipient revolution by the Left. In Europe, however, the United States and England, aided quietly by the Soviet Union, quelled the force of change and succeeded in preserving a capitalist society. In Asia, the forces of revolution triumphed in China and are still on the march today, notably in Vietnam.

Reduced to its essence, the Kolko thesis is a simple reversal of the orthodox interpretation of World War II. Instead of an accomodating United States betraying democracy in Eastern Europe to placate an aggressive, expanding Soviet Union, Kolko portrays a cautious and conservative Russia selling out the Left in Western Europe to an Anglo-American combination bent on re-imposing the Old Order. The pattern

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was set in Italy in 1943, when Stalin acquiesced in the Western plan to freeze Russia out of the Italian occupation. The Soviet leader cooperated not only because he intended to duplicate the Italian formula in Eastern Europe, but also because he sincerely wished to convince the Western powers that he did not desire to bolshevize all Europe.

Implicit in Kolko's analysis is a second, and even more remarkable reversal of traditional historical interpretation. Rejecting the conventional idea of a naive United States concentrating exclusively on the military objective of winning the war, Kolko argues that American leaders consistently and consciously fought the war to further their postwar objective of a *Pax Americana*. The Soviets were the innocent ones, fighting for survival against Germany, eschewing politics and seeking in every way possible to prevent the break-up of the wartime alliance with the West. Thus the Americans, acting out of a "higher realism" (p. 28), rejected Churchill's plan for a thrust from the Adriatic into Central Europe because it might have enabled Russia to liberate all of Germany; thus Roosevelt insisted on Great Power status for China in the United Nations because he wanted a puppet the United States could control on the Security Council; thus American leaders opposed De Gaulle throughout most of the war because they wanted a more compliant regime to rule postwar France. Rejecting the myth of a Communist conspiracy to dominate the world, which he claims existed only "in a few Leninist tracts and the minds of frightened Western politicians," Kolko replaces it with a portrait of the United States intent on "the creation of a world economy modeled after the ideal American image" (p. 323). The new conspiracy thesis, which seems to exist only in a few State Department memoranda and the minds of new left historians, is hardly more credible than the old.

Kolko is nothing if not provocative. Scholars of World War II will be astonished to learn that Cordell Hull controlled wartime American foreign policy (p. 244); that Roosevelt was a pliant, impressionable, and essentially weak leader who was the prisoner of his advisers, notably Henry Stimson (pp. 348-49); that Churchill was a Machiavellian diplomat who duped Stalin as well as Roosevelt (p. 25); that the Morgenthau Plan, while in part aimed at punishing Germany, was also "a plan for de-Bolshevizing Russia and of reintegrating it into a new capitalist world economy" (p. 323); that Stalin granted more to the West than he asked for in return at Yalta (p. 368); that Mao, while a great strategist and tactician, ranks even below Stalin as a thinker (p. 241); and that all leaders in World War II, Axis, Western and Communist alike, were irrational and profoundly immoral (p. 595).

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This last assumption, which runs throughout Kolko's book, gives the clearest insight into his point of view. Kolko is a new left populist who believes that goodness resides solely in the masses. In his opening paragraph he proclaims that when he refers to the foreign policy of a nation he means only that of the leaders, never of the people, who, he states, "were the object of worried attention, manipulation, and, in many places, physical restraint, but nowhere were they consulted on the contours of the policy of any state" (p. vii). By *people*, Kolko usually means the Left, a term which he uses without ever defining what it signifies. The Left is normally synonymous with the Resistance, but not with the Communists, who continually betray It. The Left is Asian masses struggling to overturn colonialism; the Left is ELAS in Greece, Tito in Yugoslavia, Beneš in Czechoslovakia. The Left, we are told, is both nationalistic and radical, but beyond that, it remains a vast abstraction without form, shape, or substance.

The most disturbing feature of Kolko's book is its superficial appearance of massive scholarship. It is indeed based on a wide range of published materials, ranging from memoirs and published documents to monographs and interpretive accounts by historians. His research in manuscript sources is much more limited, resting heavily on the rich Stimson collection at Yale and the Grew Papers at Harvard. Kolko does not use the State Department archives, nor does he cite the Cordell Hull Papers in the Library of Congress, omissions difficult to reconcile with the importance he places on Hull and the State Department in shaping American foreign policy. Even more disturbing is his cavalier dismissal of the prevailing historiography of World War II whenever it contradicts his thesis. The controversy over unconditional surrender is unimportant, since only Roosevelt believed in this policy (p. 373); the debate over the decision to use the atomic bomb in 1945 is "artificial" (p. 538), since there never was any doubt from the inception of the Manhattan Project that the bomb would be used. In essence, Kolko writes as if a whole body of historical literature is irrelevant to an understanding of the politics of World War II.

Kolko's book, despite its excesses, may prove to be a valuable catalyst. His sweeping judgments and dogmatic assertions should enliven many a dull seminar and provide topics for dozens of doctoral dissertations. His analysis of the conservative nature of Stalin's diplomacy and of the wartime rivalry between the United States and England, particularly on economic issues, deserves careful consideration. For campus militants, Kolko's book will become a bible, to be cited again and again to prove the iniquity of the Super Powers and the goodness of the Third World.

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For the more cynical, it will be discounted as one more effort to rewrite history to fit the contemporary mood. Those who approach it with an open mind may find that it will help provide new insights into the tangled history of World War II.

ROBERT A. DIVINE
The University of Texas

Roosevelt and World War II *by Robert A. Divine. Baltimore, The Johns Hopkins Press, 1969.—x, 107 pp. $5.95.*

The writings on Franklin D. Roosevelt's foreign policies are so lengthy and so widely varying in their interpretaions that it is an onerous task to search one's way through them to careful conclusions. Few except specialists in recent diplomatic history are likely to do so. They, too, will welcome Divine's succinct, fair, and illuminating evaluations in these four essays, the Albert Shaw Lectures in Diplomatic History, delivered at The Johns Hopkins University in April 1968. In these lectures, Divine sets forth the main outlines not only of Roosevelt's policies in World War II, but goes beyond the title to provide early background on Roosevelt and gives considerable attention to the New Deal years. He follows the interesting technique of setting forth major interpretations on each key issue, then chooses among them or sets forth his own viewpoint. His command of the complex materials is impressive and his judgments would not be easy to fault.

The Roosevelt of the controversial thirties seems to Divine basically an isolationist who, beginning with the Quarantine Speech of 1937, pursued a rather confused path toward the brink of war. Divine detects a significant pattern of action emerging from his policy in the spring and summer of 1940:

> Confronted by a major crisis, he makes a bold and forthright call at Charlottesville for a policy of all-out aid short of war. But then, having pleased the interventionists with his rhetoric, he immediately retreats, turning down the French appeal for intervention and delaying on the British plea for destroyers, thus reassuring his isolationist critics. Then, as a consensus begins to form, he finally enters into the destroyer-for-bases deal and thus redeems the pledge he had made months before at Charlottesville. Like a child playing a game of giant steps, Roosevelt moved two steps forward and one back before he took

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> the giant step ahead. Movement in a straight and unbroken line seems to have been alien to his nature—he could not go forward until he had tested the ground, studied all the reactions, and weighed all the risks.

And yet Roosevelt had to be pushed into that last giant step; it took a Pearl Harbor to bring the United States into all-out war.

It is in his discussion of Roosevelt's attitude toward collective security that Divine breaks most sharply with other interpretations. He points out that even as a young Wilsonian, Roosevelt was ready to accept the Lodge reservations to the League Covenant and that well before the 1930s he had decided the League did not work well and he no longer favored American entrance. During World War II, even while he was winning a lasting reputation as a prime proponent of the United Nations, he was holding firmly to the view that the preservation of the peace would be primarily the task of the great powers, the "Four Policemen." Finally, in a fresh analysis of Roosevelt's relations with Russia, Divine concludes that he was not as naive or soft as has often been charged, but a good deal more of a pragmatist. The serious miscalculation with Russia, Divine feels, came over the ambiguous agreement at Yalta for establishing a coalition government in Poland and the subsequent interpretations of that agreement; the quarrel over Poland made the cold war inevitable.

Overall, Divine gives Roosevelt passing but not exceptional marks for his conduct of foreign policy.

FRANK FREIDEL
Harvard University

Thirteen Days: A Memoir of the Cuban Missile Crisis *by Robert F. Kennedy, introductions by Robert S. McNamara and Harold Macmillan. New York, W. W. Norton & Company, 1969.—224 pp. $5.50.*

There is nothing substantially new in Robert Kennedy's book on the Cuban missile crisis. The Cuban missile crisis decisions were well made by the Kennedy team. I believe, however, that the administration greatly overestimated the danger of nuclear war—even if Khrushchev was a gambler, he was hardly so reckless to have risked nuclear war during a period of significant American strategic superiority—and that the Soviet Union could have been forced into a much greater retreat in Cuba

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without significant nuclear danger. Still, the president would have had responsibility for the death and destruction had he been proved wrong. If he avoided even minimal risks and yet was able to get the missiles out of Cuba, it is very difficult to argue that he blundered. He was setting a precedent for the future. He self-consciously provided face-saving retreats to the Russians in an effort to minimize risks and to establish the point that certain aspects of the nuclear status quo could not be changed unilaterally during a period of approaching quasi-parity.

The Soviets had secretively and rapidly begun a missile buildup in Cuba while denying both publicly and in private communications to Kennedy that they were doing so or would in the future. The president had publicly committed himself by reliance on these assurances. The emplacing of the missiles was occurring during an election campaign—a period during which the president presumably would be politically embarrassed were he to admit that he had been wrong and his opponents correct. Indeed, the subject remains embarrassing to relicts of the Kennedy administration who try to explain away Keating's evidence and the unwillingness of the administration to hear evidence of the buildup until it was incontrovertible—an unwillingness which, as in the case of the Bay of Pigs, made it very difficult for military or intelligence services to provide the information the president needed.

Had the Russians succeeded in their gambit—whatever their motivations actually were—a number of consequences would have ensued. The military threat to the United States would have appeared to be enormous. The ability of the Russians to change the nuclear status quo in the American backyard would likely have had very large repercussions in Latin America and indirectly elsewhere in the world. There is reason to believe that Khrushchev had badly underestimated Kennedy after the Vienna meeting. A failure in this crisis would have reinforced that interpretation of Kennedy and might have led to a Berlin crisis in circumstances in which the various misinterpretations and miscalculations might easily have produced a nuclear war.

Within this context, the charge that Kennedy escalated the missile crisis and risked nuclear war for purposes of the 1962 campaign takes on the quality of paranoia that one expects from the *New York Review of Books*. No doubt Kennedy thought of the elections. He was a politician, he had problems of regime control, and motivations are always mixed. Nonetheless, the easiest course for him in facing the 1962 elections would have been to deny the missile buildup until after the elections. On the other hand, he could not reveal the information before the elections without adverse political consequences unless he gave the appear-

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ance of effective action. (Even so, it was far from clear that he would emerge as well as he did.) In this case, however, his political interests and the national interest coincided. The worst outcome in this case for Kennedy, for the United States, and for the prospects for peace would have resulted from indecisive and ineffective actions against the Russians. The course of action he took minimized the risks of conflict and maximized the prospects for agreement with the Russians both in the short and in the intermediate term. Although I have never been an excessive partisan of the test ban and of the détente that followed, there seems little reasonable doubt that the policies pursued by Kennedy during the missile crisis stabilized the international situation. They also probably removed the immediate threat to Berlin. I am no defender of John F. Kennedy or of his family and by the time of his death I had become disaffected politically. I am startled, however, that the very actions that reduced the risk of nuclear war and that produced a period of détente have become the basis for the charge by some that he risked nuclear war for the sake of the midterm elections. The handling of the Cuban missile crisis was John F. Kennedy's shining hour. Those who make abominable criticisms of him for his handling of that crisis need to have both their heads and their hearts examined.

MORTON A. KAPLAN
University of Chicago

Gulliver's Troubles, Or The Setting of American Foreign Policy *by Stanley Hoffmann. New York, McGraw-Hill Book Company for the Council on Foreign Relations, 1968.—xx, 556 pp. $11.95.*

As his contribution to the "Atlantic Policy Series" published by the Council on Foreign Relations, Professor Stanley Hoffmann of Harvard's Government department sets out to "determine what the United States, with its enormous power, can attempt and expect to achieve in the Atlantic area . . . ." He begins with a detailed diagnosis of what is wrong with American foreign policy generally, drawing examples copiously from recent experiences like the Vietnam war, the Dominican crisis, and the disarray within NATO. America's failure to understand the implications of the shift in the international system away from bipolarity, together with procedural and institutional hindrances, have, in Hoffmann's phrase, put "Gulliver in the chain gang." Much as this metaphor would

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have mystified Swift, the author employs it to highlight a familiar, if still troublesome, phenomenon: the recurring inability of the most powerful nation in history to accomplish its goals beyond its own borders.

Most of the entries in his long catalog of defects in American policy-making are well known. Hoffmann's critique is perhaps most novel in its treatment of what he calls America's "national style." Elusive and difficult to measure as the concept may be, Hoffmann insists that often its role is pivotal in determining the success or failure of national policy.

The heart of the book, however, is the shorter concluding section. Here, in a lengthy case study, the author concentrates on America's relations with Europe. Again, his identification of the problems and tensions besetting the Western community contains few surprises. What sets his analysis apart from a number of other books dealing with this subject is that Hoffmann endeavors—with a notable degree of success—to expose the basic *sources* of these disorders, by separating symptoms from underlying causes. Having done so, he offers his own proposals for achieving a new era of cooperation among the nations of the West, not excluding thte Communist nations. Hoffmann's policy recommendations are too detailed to summarize in a brief review. It must suffice to say that—after evaluating, and wholly or partially rejecting, proposals made by other officials and commentators interested in the same problem—he calls for the creation of a politically unified "European Europe" extending from Great Britain to Poland. (Hoffmann's scheme, it is essential to note, differs fundamentally from the Gaullist idea bearing the same label.) Initiated by the Europeans, but vigorously supported by the United States, the emergence of this new political entity would, in the author's view, ultimately solve (or at least attenuate) such seemingly intractable problems as the division of Germany, Britain's relations with the continent, the reformulation of Western nuclear strategy, and French isolation.

It is a cogently argued, and in many respects highly persuasive, idea presented by a scholar who obviously has a detailed grasp of his subject and a gift for effective communication. In contrast to some books with a thesis, Hoffmann's study is characterized by objectivity, balance, and fairness to opposing views.

If there is a significant criticism to be made of Hoffmann's plan to "untie" Gulliver, it is that—on the basis of his own evaluation of the impediments and restraints facing American policy-makers—he underestimates the difficulties of achieving his goal. Successful realization of his scheme would require nothing less than a revolution in American foreign policy. For good or for ill, many of the elements present

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in America's "national style" and its foreign-policy process are so deeply embedded in history and tradition as to render their wholesale or rapid elimination highly improbable. Yet the indispensable first step in their removal is, admittedly, the kind of dispassionate examination of the problem we have received in this book.

CECIL V. CRABB, JR.
Louisiana State University

European Economic Integration and the United States *by Lawrence B. Krause. Washington, D.C., The Brookings Institution, 1968. —xiv, 265 pp. $6.75.*

The United States has supported consistently the integration of Europe, largely for political reasons. For we have believed in the application of the principle of federalism to Europe and especially its value in binding Germany firmly into Western Europe. As to economic consequences, it was assumed that economic benefits to this country would follow as a result of a more vigorous European economy. Mr. Krause has made a careful analysis of this reasoning and the results as they are revealed in the available statistics.

To squeeze the meaning out of the available statistics, Mr. Krause has applied about every test that he can devise. He acknowledges that the documentation, "is more akin to circumstantial evidence than to proof. Because of the nature of the question, cause and effect relationships have been inferred rather than observed" (p. 216). But even with these qualifications, the author's ingenious analysis of trends of exports and imports in relation to past trends, to production, and to investments, broken down by areas and by periods, builds up a reasonably convincing case.

The outstanding fact is of course that since the establishment of the two trade areas the aggregate of trade has increased tremendously. As to manufactured goods, the policies of the E.E.C. have resulted in far greater increases in trade among members than with non-member countries including the United States. The U.S. percentage is substantially reduced. With the E.F.T.A. countries, the United States has held its percentage about constant. In both cases the rapid increases in total trade probably reflect over-all increases in income more than tariff policies. The stimulus to enterprise from integration and from increased invest-

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ment was partly offset for U.S. trade by tariff discrimination. To this extent one can say that the U.S. has been injured.

The case of agricultural products is quite different. The E.E.C. agricultural policy is seriously protectionist. As successive steps in a common agricultural policy have been taken, the extent to which the United States is damaged has become clearer. The author tried to put a price on this loss and comes to a figure of $215 million to $265 million a year. For the world as a whole the amount may be two or three times as large. The full effect of this comes at a time when the E.E.C. countries have been running a surplus in their balance of payments and the United States and the United Kingdom serious deficits. Thus while the amounts of damage done are not large in relation to total trade, they hit a vulnerable spot.

The Kennedy Round, the author believes, brought some net benefit to the United States in terms of freer movement of manufactured goods but was little help in liberalizing trade in agricultural products. There perhaps was some prospective benefit in an agreement to share the financial burden in supplying temperate agricultural products to less developed countries.

So the author's careful analysis brings him back to the judgment that the benefits to the United States of European integration are much more political than economic. Europe is very important to the United States as the only region capable of sharing the responsibility of security, of rendering economic aid to countries outside the Atlantic Community, and of seeking better relations with Eastern Europe. The principal regret is that these hoped-for political gains have been no greater and that the E.E.C. has fallen so far short of its political objectives.

W. RANDOLPH BURGESS
The Atlantic Council of the United States
Washington, D.C.

The Psychological Dimension of Foreign Policy *by Joseph H. de Rivera. Columbus, Ohio, Charles E. Merrill Publishing Co., 1968.—vi, 441 pp.*

It is altogether but sadly appropriate that this review is being written as Russian tanks are moving into Czechoslovakia (one wonders whether they will still be there when the review is published). For it seems doubt-

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ful that the Soviet venture, with ramifications reaching from Prague to Vietnam, can be entirely explained in terms of rational considerations, whether Russian interests, bloc requirements, game theory, or whatever. Rather it appears that there is an irrational, not to say mad, element involved in the Russian invasion of a country that, despite its increasingly independent mood, remained in essentials a client state of the Soviet Union. Almost certainly the Russians have gained nothing from their action, but it is already clear what has been lost: their image as the less belligerent of the two super-powers; the trend toward democratization in Eastern Europe; the solidarity of the Communist bloc; the further de-escalation of the cold war; and, quite possibly, the chances for peace in Vietnam. Whether the Democrats win or lose in November, it is clear that their prospects, and especially those of McCarthy, have not been helped by the hard-liners of Moscow.

As an aid toward understanding this latest barbarity I can think of no better book than the one under review. Professor de Rivera, of course, is not concerned with the immediate issue, nor is he even a Kremlinologist. He is, however, a superb psychologist (at New York University) of a species that is rapidly disappearing; that is, he is one of that rare breed of psychologists (others being Herbert Kelman, Charles Osgood, Nevitt Sanford, and the luminaries of the Society for the Psychological Study of Social Issues) who are interested in the larger concerns of society and politics. Indeed in this respect his book is more relevant to political science than most of the work now being done in the fields of international politics and foreign policy.

Such a rich, diverse, and creative contribution cannot be adequately dealt with in the space permitted. Suffice it to say that Professor de Rivera has not omitted any topic meriting inclusion under "psychological dimension," whether it be personality, psychodynamics of decision-making, psychopathology, communications, small group behavior, perception and cognition, leadership, public opinion, or interpersonal relations. And what is more he has an extraordinary grasp of the literature in these areas and knows how to move easily from it to practical applications, from decision-making theory to the Korean War or from organization theory to the State Department.

Finally, he is as sensitive to some of the complex nuances of our era as he is sensible to the obstacles in the way of rational world order. One of his more original and urgent conclusions is that there should be institutionalized somewhere in the government the functions of gathering information that goes against current policy, indulging in "impractical" policy planning, representing "unfriendly" nations, and scrutinizing pol-

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icies not in terms of their probable benefits for the United States alone but their benefits for the world and humanity at large. The implementation of any one of these proposals would be radical enough, the implementation of them all would transform the policy process and with it the international condition. But is anyone in high office even thinking about such matters? Assuredly not the Russians, or Chinese, and probably not the Americans who replace Johnson, Rusk, Clifford *et al.* Nor will they until it is too late.

ARNOLD A. ROGOW
City University of New York

Alliances and Small Powers *by Robert L. Rothstein. New York and London, Columbia University Press, 1968.—xiv, 331 pp. $10.00.*

To go no farther back than the nineteenth century, the distinction between Great and Small Powers has long been an accepted one. Clearly, this implies a distinction in the range of power, a factor which is itself the resultant of a multitude of components that the author makes no attempt to analyze, list, or evaluate. Endeavoring to find a definition, he adopts the criterion that "a Small Power is a state which recognizes that it cannot obtain security primarily by use of its capabilities." Leaving aside some obvious shortcomings of this definition—was France, for example, a Great Power in the nineteen-thirties?—it leads to the conclusion that the Small Power "must rely fundamentally on the aid of other states, institutions, processes, or developments to do so." Hence the title of the book whose object is to examine the use, the operation, and the efficacy of alliances as used by Small Powers. Hence also the division of the work into three parts, a preliminary theoretical discussion followed by an analysis of the nineteenth and twentieth century records and closing with a survey of the contemporary scene.

The first two sections overlap, for the analysis of two specific instances, the Belgian case, especially the Franco-Belgian relationship in the period 1919-1936, taken as an illustration of an unequal alliance, and that of the Little Entente as one of intra-Small Power alliances, is again touched upon in the second part. A certain amount of perhaps unavoidable repetition is the consequence.

The observation that "alliances have proved highly resistant to theorizing which proceeds much beyond translating common sense into po-

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litical maxims" is eminently sound; it explains the limitations of the discussion which indeed contains common and valuable observations but does not succeed in furnishing a novel or very useful basis for past analysis or future action.

That an unequal alliance tends to produce a relationship of dependence on the part of the lesser partner, which would naturally prefer it otherwise, can hardly be gainsaid. But neither does neutrality or nonalignment, distinct but related concepts, necessarily serve the Small Power any better. The author considers the Little Entente a successful combination, but correctly points out that it was only so so long and to the extent that it concentrated on limited aims and while it remained immunized from Great Power conflicts. The last section raises the question of the impact of technical developments, a factor previously at work though to an incomparably lesser degree than at present, on the very concept of Great and Small Powers. The general observation that Small Powers tend to gain in security what they lose in influence and independence is well warranted.

Some generalizations may appear rather loose. How committed was Britain to fight on the French side before 1914? Can one speak of Italian irredentism as a threat to the Little Entente? To regard as inevitable a Franco-Italian understanding in 1935 at the expense of Yugoslavia needs at least qualification. Other examples could be given. And one can only regret that a French citation or title free of typographical error is the exception rather than the rule. There are nevertheless valuable insights in this discussion.

René Albrecht-Carrié
Barnard College

The Professional Diplomat *by John Ensor Harr. Princeton, Princeton University Press, 1969.—xii, 404 pp. $11.50.*

This is a well-documented study of a subject different from that indicated by its title. It would be more accurate to call it a sociological analysis of the foreign affairs establishment of the United States. Within that frame of reference this is a good book.

There are allusions to Sir Harold Nicolson and the Plowden report. To that extent there is awareness of a British foreign service. But there is no hint that France, Germany, the Soviet Union, and other nations have problems with their professional diplomats. Emphasis is laid upon

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the distinctive culture of our foreign service officer corps, but no reference to the cultures of other diplomatic corps, whether they have an elitist character, nor to their recruitment, promotion, career management, and retirement. Comparisons with experience outside the United States would have been useful.

The time perspective is very short. There are, indeed, references to the Rogers Act and the Foreign Service Act of 1946 and also mention of earlier studies of the foreign service. The discussion is centered upon the sixties, and more particularly upon the Herter report of 1962 and the brief period when Deputy Under Secretary for Administration William J. Crockett undertook to introduce modern management techniques into the Department of State.

Emphasis is laid upon a definition of the "'new diplomacy," by which the author means the impact of our policies upon a larger number of nations and the vastly greater range of such impacts—political, economic, social, cultural, military, and others. It would have been illuminating had he applied the criteria of the "new diplomacy" to other nations. Is such a redefinition as significant for their foreign affairs establishments as for ours?

The foreign service officer corps is analyzed statistically and otherwise in great detail. Inevitably the obvious is sometimes elaborated, but there are also fresh insights. Brief note is made of the problem of recruiting Negro F.S.O.'s, but, astonishingly, the other "disadvantaged" group, women, is not so much as mentioned. Unfortunately there is no adequate exposition of the historical reason for, nor any real analysis of, the misnamed Foreign Service Reserve. The discussion of lateral entry, its rationale, its history, the reasons for its successes and its failures, is too brief to be enlightening.

Management, the key to success in the new diplomacy, gets thoroughgoing and valuable treatment. One key fact is brought out clearly—that we have not had secretaries or undersecretaries with experience in the field of modern management or any profound interest in it. No matter what innovative and valuable suggestions are proposed by such groups as the Herter committee nor what energy and inventiveness a deputy undersecretary, like Mr. Crockett, may pour into the effort for reform, their effect is transient unless the secretary of state believes, not routinely but profoundly, in their validity. So far this has not happened.

The question whether the Department of State should be operational or policy directed is explored but with too little attention to the way in which the personal idiosyncracies of successive secretaries have affected the judgment.

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The argument in favor of a Foreign Affairs Service of the United States is valuable. It would bring under one personnel system all who represent the nation abroad in every phase of the new diplomacy. It would have been helpful to describe efforts that have been made to exchange personnel among relevant departments in an attempt to build up a cadre of professionals whose departmental myopia has already been corrected to some extent.

Nowhere does the author discuss the world experienced nettle of the relationship of the bureaucracy at home—the *sedentaires* as they were called in France—and the "exiles" who spend their lives abroad. Nor is the complex relationship of the civil service and the foreign service analyzed in depth.

There is an enlightening treatment of the breach in the traditional culture of the diplomat by an activist group of younger officers. They are now pressing for reform in the areas where the corps of professionals has habitually been most resistant.

HENRY M. WRISTON
New York City

The Emergence of Indian Nationalism: Competition and Collaboration in the Later Nineteenth Century *by Anil Seal. Cambridge, Cambridge University Press, 1968.—xvi, 416 pp. $11.50.*

Most books on Indian nationalism fall into one of two categories: those on some reasonably defined segment by a writer who is expert in his area but who does not undertake to locate his theme in the mainstream of Indian social and cultural history or those on the whole nationalist period, usually written by people innocent of either special or general knowledge. This book, which the dust-jacket promises to be the first of a series on political change in India from the 1870s to the 1940s, is an exception. The author's theme is broad, but supported by much careful research, and the historical realities are always present in the background to give meaning to the analysis. And as a bonus, whose value will be fully appreciated only by obsessive readers of books on India, the author writes extremely well. If he has not produced the most important book in recent years on Indian history, he has certainly written the most interesting and readable one.

Seal begins his study with the axiom that the history of any colonial system is a series of permutations between government and different sets of allies. The collaborationists, a word he uses in a non-pejorative sense,

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made possible a form of government that maintained itself not just through the passivity of the majority of its subjects but through the active support of important groups. These comprised the army, two-thirds to three-quarters of which were Indians; landlords; businessmen in the great new port cities; above all the bureaucracy, almost wholly Indian beyond the thousand or so civil servants; and the new professionals created by Western rule in law, medicine, college teaching, journalism.

Seal shows how these last two groups, the bureaucracy and the new professionals, passed from being collaborators to competitors for power. Bengal is inevitably the major geographic focus of this process, since nationalism was articulated there and Bengal provided the leadership throughout much of North India in the bureaucracy and the professions. But Madras and Bombay receive more careful treatment than is usual in books on the emergence of nationalism. Ripon's administration is used to exemplify the ambiguities and dilemmas of the rulers, and Seal correctly demonstrates how many policies and conflicting interests were involved. The British Cabinet and the Indian Council in London were not at one, and Gladstone had little time to give to the support of his Liberal Viceroy; in India, the civil service opposed Ripon as did the non-official British community. Nor was there anything like unanimity among politically aware Indians, and Seal's treatment of what he calls "the Muslim breakaway" is concise and objective. Only once or twice does he speak of the fire and sword of Aurangzeb or the fanaticism of Muslims. The rise of Muslim nationalism is accepted as a normal outgrowth of the political process that produced what the Indian National Congress identified as "Indian nationalism." What is astonishing, and the true measure of the success of collaboration and competition, is that, by and large, only two nationalisms emerged, thus confounding those who argued that India was not one but a dozen nationalities.

A criticism that may be made of Seal's work is that perhaps he does not give enough attention to the importance for Indian nationalism of what, for lack of a better term, may be called the infrastructure of bureaucratic imperialism. The army, the civil service, the railways, the economic development which was greater than is usually implied, the foreign policy of the government of India, its monetary policies, all these were important for the particular results of collaboration and competition. But the themes he does choose to discuss are so well handled and his judgements are so sensibly expressed that one can safely predict that this will soon become the most useful introduction to the study of Indian nationalism.

AINSLIE T. EMBREE
Duke University

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The Arab-Israeli Dilemma *by Fred J. Khouri. Syracuse, N.Y., Syracuse University Press, 1968.—xi, 436 pp. $10.00.*

This book contains 355 pages of narrative, an appendix of documents and statistical tables, extensive notes with references to hundreds of publications, and a sixteen-page bibliography. Despite such imposing scholarly apparatus, this historical account of the Arab-Israeli conflict is not satisfactory. Although there is considerable detail, in presenting factual summaries of certain events, such as UN debates, it often fails to consider key documents and strategic factors. For example, Mr. Khouri fails to consider Sir Henry McMahon's letter to *The Times* of London of 23 July 1968, in which McMahon gives an emphatic interpretation of his own letter of 24 October, 1915 to Sharif Hussein of the Hejaz, one that differs sharply from the interpretation of Mr. Khouri.

The first four chapters take the story up to the UN Resolution on Partition of November 29, 1947 and the Palestine War of 1947-49. A relatively short chapter on Jerusalem is followed by longer discussions of Arab refugees, the Sinai War of 1956 and its aftermath, and the June War. The book concludes with a review of efforts since 1949 to bring about a peace settlement, the current positions of Israel, the Arab states, the United States, and the Soviet Union, and suggestions for making progress toward a peace settlement.

The author fully understands that the root cause of the Arab-Israeli conflict lies in the incompatible claims of the parties to Palestine arising from nationalist forces and the blunderings of the great powers. He blames all parties to the conflict for contributing to its perpetuation, but he establishes no order of significance or weight in allocating blame. However, Israel tends to be the principal obstacle to peace throughout this study. He is highly critical of Arab leaders for their lack of political maturity and foresight and for ineptitude and disunity. He repeatedly scores Arab communities for failing to organize effectively, develop unity, or produce effective leaders. A basic charge against the Jews and Zionists is their serious lack of understanding of the existence and intensity of Arab national pride and a consistent disregard of Arab feelings. He is offended by the skill of the Zionists in "international propaganda" and exhorts the Arabs to emulate the Zionists. His language characterizing American Zionist influence in the Mandatory period is tough in contrast to his characterization of the Mufti's pro-Nazi position as "naive" and "misguided." He expressed concern for the murder of Jews in the Holocaust, but gives no figures indicating the enormity of their destruction.

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One of the weakest aspects of the book is the treatment of American policy in the Middle East. He very superficially describes the American position as the politically expedient product of intense American Jewish pressure on partisan political leaders. The word "oil" does not appear in the index and occurs rarely in the text. The strategic factors in United States policy are largely ignored, and the existence of an oil lobby is never acknowledged. Mr. Khouri sees the United States government moved by popular emotions and partisan politics instead of by informed appraisals by professional diplomats and military officers of long-range interests. The latter remain undefined.

Again and again the author stresses that overwhelming military defeats have dealt terrible blows to Arab pride and self-confidence, which in turn has created major instability and unrest. In Israel, he finds that the major effects of military victories are a significant growth in "military mentality," a willingness to use any methods to achieve ends, a "supreme devotion to self-interest," cynicism, and a propensity to use armed power (pp. 99-101). In twenty pages of text, the reader is not informed that Jordan won control of the Old City of Jerusalem as part of the spoils of war. Perhaps the major charge made against Israel is its refusal to embark on a program of repatriation and/or compensation of refugees. While he states that there is "some validity" to the Israeli claim that Arab officials have played politics with the refugee problem (p. 162), he finds in Israel's refusal to carry out UN resolutions on refugees sufficient justification for the Arab position on almost every point of conflict with Israel.

Mr. Khouri believes that Israel has repeatedly exaggerated Arab threats to its survival, used alleged Arab provocations to justify massive retaliations, and engaged in provocative military actions in May and June 1967 because it confidently expected the United States to intervene in her behalf. Arab threats are criticized only as misguided, bellicose, blustering, and unduly intemperate.

While the UN comes in for a share of the blame, the author is inclined to depend heavily on the UN to work step-by-step toward partial solutions of the conflict. He cites Israel's distrust of the UN, but never explains the reason for her hostility. The treatment of the June War, although lengthy, is skimpy on analysis. Far more space is given to summaries of official positions in UN debates than to an analysis of the positions and actions of the United States and the Soviet Union. Only two pages are devoted to an "analysis" of the role of the Soviet Union.

Most chapters contain "conclusions" that often do not follow from any of the preceding materials. For example, the author "concludes" that

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as of early 1968: "Because of popular passions, internal instabilities, and pressures from extremists, even the moderate Arabs did not dare go too far in yielding to Israeli demands" (p. 346). But there is no preceding discussion of the passions, instabilities, and extremist pressures. In addition, economic and strategic factors are ignored throughout, and ideological, religious, and other influences affecting the prospects of political modernization in Arab countries are neglected.

MARVER H. BERNSTEIN
Princeton University

The Mandate of Heaven: Record of a Civil War—China 1945-49 *by John F. Melby. Photographs by Henri Cartier-Bresson. London, Chatto and Windus, 1968.—ix, 313 pp. $10.00.*

In the State Department's *U. S. Foreign Relations* series, no volumes have yet been issued for the Chinese civil-war period, 1945-49, and the American public is limited, for official documentation regarding the "controversial" events of those years, to the 1949 "China White Paper," *United States Relations with China With Special Reference to the Period 1945-1949*. There is a profound gap in our knowledge of what went on in official circles during that critical time. There were of course Americans who were directly involved in those events. Dr. John F. Melby, Secretary of the U. S. Embassy in China from late 1945 to the end of 1948, was one such. His book *The Mandate of Heaven*, composed of excerpts from his contemporary diary with an interlarded text expanding upon the overall theme, is a firsthand account which contributes substantially to our understanding of the American relationship to the Chinese embroglio of the postwar period.

The partisan American theses circulated in the 1950s suggested that China was "lost" to the United States by American treason or by Washington's failure to employ military force to impose a satisfactory settlement on China or, at best, by political ineptitude. The Melby book establishes the causes of the Communist overthrow of the Nationalist power as having been very different. In his account, individuals play historically significant roles. General George C. Marshall, sent out as special envoy to take up the task of mediation between the Nationalist and Communist factions abruptly dropped by Ambassador Patrick J. Hurley, won early success by stage-managing a truce in the civil war that had already begun, only to see that accomplishment insidiously undermined

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by the antagonists. Marshall was frustrated and angered by the maneuvers of Chiang Kai-shek in particular. He finally came under attack from both Chinese sides, and it was against his better judgment that he stayed on past the point of meaningful endeavor. Ambassador J. Leighton Stuart, long-time missionary-educator, is shown assuming well after the last hope had faded that he would be able to work something out *à la chinoise* between the warring factions. Then there was Lt. General Albert C. Wedemeyer, dispatched by President Truman as head of a mission to assess the overall situation in China in the summer of 1947. He submitted a report highly critical of the Nationalists, and later swung into the pro-Nationalist camp. Melby remarks that various other Americans acted in the same pattern.

In action throughout the story is Chiang Kai-shek, the stubborn military dictator who thought his military and political judgment infallible. It is evident that Marshall's estimates of the military position and of the political requirements of the situation were superior to those of the Generalissimo. Chiang however proposed to manipulate the United States to serve his own political ends and disregarded American advice directed toward a different general goal. The author's overall picture portrays a faction-ridden Nationalist regime that was corrupt and ineffectual. One derives a vivid sense of the progressive decay of the economic situation in the postwar period; alongside that development there was the rapid decline of the Nationalist political and military power. The Nationalists, however, confident that the United States would continue its support, refused to give serious consideration to the establishment of a coalition government to replace their one-party dictatorship and aimed at a military victory over their opponents. The war went on to its ultimate conclusion—the collapse of the overextended "modern" Nationalist military machine under the battering of Communist peasant armies. Thus the Mandate of Heaven passed in China.

O. Edmund Clubb
Columbia University

Tanzania: Party Transformation and Economic Development *by Henry Bienen. Princeton, Princeton University Press, 1967.—xv, 446 pp. $11.50.*

Professor Bienen feels that students of African politics have tended to accept too hastily African images of African politics and to confuse intentions with facts. Studies of African one-party systems reflect this ten-

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dency. These party-systems have been described in terms of typologies such as "mobilizing systems" and "revolutionary-centralizing systems." While these typologies reflect to a considerable extent what the exponents of the one-party system want their party to be, they are not helpful for understanding the actual nature and role of these parties. For one thing, the typologies lead one to expect that these parties are tightly and hierarchically organized and that their authority is pervasive. Such expectations are quite misleading.

The criticism implies that it is necessary, or at least desirable, to take a more careful look at how these parties are organized and how they work. And this is what Bienen has done. His book is a study of the Tanzania African National Union (TANU), and he tries to show how TANU might be better understood by watching it closely as it interacts with the society that it is seeking to transform both economically and politically.

The study is organized into four parts. The first part examines the evolution and role of TANU before Tanzania became independent. Part two is an exposition of the organizational structure, recruitment procedure, and ideology of TANU. I found the first three chapters of this section particularly insightful. Part three, entitled "TANU and the Economy of Tanganyika," gives some idea of how the party interacts with its societal environment, particularly the villages and districts. The fourth and final section is somewhat like an appendix; it describes two events, the army revolt of January 1964 and the elections of September 1965, which the author regards as dramatic illustrations of the character of the party.

What emerges from all this is the conclusion that TANU "does not provide an institution which can transform the economy and make itself more effective in the process; it is too weak and too loose and has too few material and human resources to tackle developmental problems." TANU "cannot become a tightly disciplined, hierarchically organized party which rules from the center without first effecting fundamental changes in the relationship of the party to economic tasks and without structural changes in the economy taking place. There will have to be a shift away from the present ratio of subsistence to monetary sectors, a change in the proportion of subsistence farmers to total working force, a rise in the level of productivity and per capita income."

Professor Bienen's argument that TANU is loosely organized and that its hold on Tanzania is uncertain is convincing. However, the significance of debating whether the organization of TANU is hierarchic or not or whether it is adequately described by a particular typology is not entirely clear. It is fortunate that Professor Bienen is much more interested in telling us how TANU works than in carrying on this debate.

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The study implicitly raises an important and interesting question: what are the conditions under which dominant political institutions (especially the single-party system) succeed in transforming their environments (especially their economic environments), and how does the environment limit the capabilities of political institutions. There are indications that Professor Bienen is inclined to the widely current but demonstrably false view that the capability of political institutions varies with the level of economic development of the relevant society. This view fails to see the relational nature of power. Perhaps it would be more to the point to say that Professor Bienen does not explore this question. His contribution is more modest. It lies in his detailed description of the structure and the working of TANU. The study makes both the politics of Tanzania and the phenomenon of uni-partyism more intelligible.

CLAUDE E. AKE
Carleton University

Population and Political Systems in Tropical Africa *by Robert F. Stevenson. New York, Columbia University Press, 1968.—xii, 306 pp. $10.00.*

The author takes up a statement made by Fortes and Evans-Pritchard in *African Political Systems* (1940) that "it would be incorrect to suppose governmental institutions are found in those societies with greatest densities. The opposite seems to be equally likely judging by our own material." Stevenson rightly points out that this position is different from that generally found in Eurasia and America. In conformity with a view of the state as an "adaptive" institution, emphasized by his intellectual mentors at Columbia, he sets out to show that the authors of the above statement (whom he identifies rather diffusely as the British "school" of social anthropology) are wrong with regard to some of the six peoples they use and that "the African data also demonstrates that population densities are generally higher at state than at non-state levels" (p. 7).

This he tries to do by recourse to historical sources. He has two main moves open to him. Firstly, to show that the population figures given, derived as they are from post-colonial censuses, do not reflect the pre-colonial situation. However since we have no pre-colonial census, we are asked instead to consider vague estimates of travelers and unweighted factors (such as local migrations). Secondly, he can try to show that

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the systems called "states" are less centralized than the fieldworkers claim (he offers a reinterpretation of Ibo for example) and that the "acephalous" societies are in (or in the borders of) a state. With an expandable definition of a chief, it is not difficult to show that such roles exist everywhere. After discussing the societies dealt with in the earlier book, he concludes with a general survey of Africa, carried out in a rather impressionistic way, and a more detailed consideration of the most obvious exception to this thesis, the Ibo of Nigeria.

Stevenson makes some useful points. But his work suffers from an over-intensive committment to his hypothesis and the unsystematic, indeed biased, way in which he treats his evidence. Take for example the Tallensi. He suggests that the high population density is (1) due to a recent inflow of population from the north, (2) related to its inclusion within the Mamprusi state. Even if his points were acceptable, his argument completely overlooks the outflow of migrants from Tallensi (a fact which he points to in the Bemba case when he wants to prove the opposite); and it would fail to account for the comparatively thin population in the core areas of the Mamprusi state, the very areas that should be most affected by his hypothesis. He appears to be aware of this deficiency and offers the untestable, indeed unacceptable, suggestion that the Mamprusi state originated in the stateless (high density) area.

But in any case his facts on the Tallensi are in dispute. The author hints at archival research. Indeed at one point he states that, "One has only to read such as the district reports of the period to learn that the densities of the Tallensi and the Logoli (both classified as acephalous) had been decidedly influenced by the presence of colonial state power" (p. 236). The unsuspecting reader might think that Stevenson's research was based on an analysis of just such documents. But nowhere do we find a reference made to them. One cannot help feeling that had he done what he implied, he would surely have come across the statement by the first commissioner of what was then the Northern Territories of Ghana to the effect that the Tallensi area was "thickly populated." For the extent of Mamprusi jurisdiction in the immediately pre-colonial period, even the published source he uses excludes Tallensi country from Mamprusi, giving the White Volta as the boundary [L. Binger, *Du Niger au Golfe de Guinée* (1892) II, 25] I do not know well enough the situation in other parts of Africa but the census figures of northern Ghana do show a negative correlation between "organized governments" and "barbarous tribes," to use a distinction made by C. E. Ferguson, an early observer. That Stevenson is so concerned to reject this correlation (even though an adjustment to his hypothesis could help to explain it) is an indication of

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the strength of his attachment, not to evolutionary schemes (which are to be encouraged) but to a model of the relationship between state and economy which is inappropriate to Africa because of basic differences in the productive systems. But what is more devastating to this reader is the fact that where a social scientist has made a serious attempt to marshall the evidence he should be prepared to accept standards that any historian would recognize as thoroughly inadequate. But this, I am afraid, is a comment on too much research in this field and one is left wondering how so much heat can be produced by so little fuel.

JACK GOODY
St. John's College, University of Cambridge

Nationalism in Brazil: A Historical Survey *by E. Bradford Burns. New York, Frederick A. Praeger, 1968.—ix, 158 pp. $5.50.*

Professor Burns has given scholars interested in Brazil a compact but extensive overview of nationalist currents in Brazilian thought. The author ranges widely in his scouring of the literary thickets for manifestations of nationalist ideas. Still, the book holds few surprises and makes a few striking omissions. In seeking to document the early roots and enduring vitality of the idea of nationhood as a politically motivating force, the author often seems to take at face value the affirmations of the more grandiloquent panegyrists of the nation. That is, while the judgments offered concerning the quality of the particular contributions to the nationalist literature are on the whole well balanced, the interpretation of their social significance is often conventional, matter-of-fact, or simply absent.

The book traces the development of nationalist thought through various stages—nativism ("precocious patriotism"), defensive nationalism, economic nationalism, and a final period ambiguously labeled as one of "action and reaction." The reaction in question is the military takeover of 1964, which Burns without hesitation characterizes as "a stunning blow against the growth of democracy in Brazil" (p. 114). He is less forthright with respect to the United States role in that coup, on the one hand citing U.S. sources which candidly depict the supporting actions of U.S. officials at various points, while on the other continuing to treat such information as part of a nationalist "case" against the Yankee enemy.

The book is weakest in its treatment of the present crisis of nationalist ideas. Like almost everyone concerned with this problem, Burns seems

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baffled by the collapse of nationalism as a political force in 1964. The natural tendency is to continue to talk about that changeover of power as a momentary setback or interruption in the main thrust of maturing nationalist development in the nation. This tendency at once minimizes the importance and profundity of the failure of nationalist ideologies and leadership and denies much needed attention to the new forms in which countervailing groups are seeking to restructure and consolidate an alternative political design. In this connection, the absence of any mention of the military doctrine of *segurança nacional* as a contemporary nationalist formulation comes as something of a shock.

What is troubling in the volume is, thus, the lack of a standard or design for treating the role of ideas in politics, for characterizing or comparing ideological construction, for judging their real weight in the world. The book pulls together a great many sources and engagingly documents important shifts in perspectives, in the content and style of self-affirmative definitions of Brazilian capabilities and aims. It neglects substantially the internal resistances to such affirmative and socially inclusive political formulation, the repeated political regressions in moments of crisis, the diffidence in the face of opportunities for greater autonomy. In Brazil as elsewhere the fear of anarchy has time and again proven greater than the faith in democratic ideals or the will and desire for self-management.

FRANK BONILLA
Stanford University

The View from the Barrio *by Lisa Redfield Peattie. Ann Arbor, The University of Michigan Press, 1968.—147 pp. $6.95.*

This small book is one of the most enlightening studies in the field of planning and economic development that I have read in recent years. It is concise and well written, humanistic and interesting; and yet it makes a number of highly pertinent theoretical contributions. Mrs. Peattie is not directly concerned with the growth of GNP, the population explosion, or other quantitative aspects of the process of social, political, and economic modernization. Her study, however, is explicitly related to the problems of urbanization and economic development.

Mrs. Peattie studied a small *barrio*, an urban neighborhood called La Laja, of Ciudad Guayana in Venezuela. Her *barrio* had in 1962-64 only 490 people living in 80 households. She lived there for two and a half years with her family as an anthropologist working on a project of the

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Harvard-MIT Joint Center for Urban Studies. Ciudad Guayana is a planned city, formally inaugurated in 1961, built in the tropical semi-wilderness on the Orinoco River. It has a dam and an electric power plant and a government-owned steel mill as well as subsidiaries of two North American mining companies. In 1962-63, the Ciudad had about 50,000 people but a population of 250,000 was projected for 1970 and 600,000 for 1980. Thus, the small neighborhood of La Laja does not loom very large in this enormous plan and Mrs. Peattie is quite humble about the contribution of this study to the larger plan.

In my opinion, she need not be so humble. Her book goes a long way toward explaining why planned cities (for example, Brasília) either fail or diverge sharply from the plan. Not only do the planners and builders fail to take into account the complexities of the larger society but also the concrete life situations of the people who will live and work in such cities. The planners of Brasília in Brazil did not plan the shanty towns that have sprung up around and even inside that city. With her intimate knowledge of La Laja, Mrs. Peattie has focused upon human problems. For example, her chapter on "The Sewer Controversy" is one of the best step-by-step analyses of the barriers imposed by a rigid social class structure on communication and mutual understanding between individuals and groups that I have ever read. Her description of the world view of Señor Figueres, an elderly inhabitant of La Laja, is less dramatic but equally enlightening. The book is filled with descriptions and analyses of specific human situations that impinge upon planning and economic development.

In short, Mrs. Peattie has taught me something about Latin America; she has strengthened my notions about the people involved in the process called by such grand terms as "economic development," "urbanization," "political socialization," and the like. I hope that my colleagues in economics, political science, sociology, and urban planning as well as social anthropology will read this book. It relates real people and their problems to abstract theory.

CHARLES WAGLEY
Columbia University

Guineas and Gunpowder, British Foreign Aid in the Wars with France: 1793-1815 *by John M. Sherwig. Cambridge, Mass., Harvard University Press, 1969. —xiv, 393 pp. $11.00.*

It is common knowledge that Britain, during the wars against revolutionary and Napoleonic France, granted large subsidies to her continen-

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tal allies, but this problem has never been systematically explored, and Professor Sherwig has attempted to fill this gap in historical literature. He has done a great deal of research at the Public Record Office, in the papers of the Foreign Office, Treasury, Audit Office, War Office, and Paymaster General's Office, and he has used, of course, all the main printed sources. He has thus been able to calculate a table of subsidy payments (pp. 365-68) by year and country which is undoubtedly more accurate and complete than the data previously available. Moreover, his account of the development of British "foreign aid" policy brings to light a number of interesting points. Mr. Sherwig explodes the myth of "Pitt's gold"—the popular idea of the British government throwing money around in order to create coalitions against France (unfortunately his title might conjure up another myth—the *cavalerie de Saint-George*, as the French used to say, while, in fact, the export of the gold coin of the realm being prohibited by law, guineas were not used to pay subsidies, except in a few exceptional circumstances). He proves that Britain's subsidy policy was evolutionary in nature; far from eagerly assuming the role of financier to the coalition, Pitt shunned it as long as he was able; during the early part of the wars, subsidies were used mostly as emergency measures to keep the coalitions alive and reluctant allies in the war. But a major turning point occurred with the formation of the Third Coalition, in 1804-05, when Pitt gave up this policy of selective subsidization and offered money with a free hand to any power ready to take arms against Napoleon. Then, from 1808 onwards, Britain spent lavishly in the Peninsula, mostly to maintain her own army, but also to help Portugal and the Spanish insurgents. It was only in the years 1813-14 (and also in 1815), however, that the "foreign aid program" became massive—with, according to Mr. Sherwig, a decisive effect on the military effort and victories of the coalition. Mr. Sherwig stresses also that, while no government was willing to fight France without British financial help, the offer of subsidies had generally little influence on their decision to take arms, and money, no matter how freely given, could not buy an ally's dedication to the cause. Such conclusions are quite valuable, but one would have liked more details on the machinery of transfer payments and on "the economics of subsidization," which Mr. Sherwig frankly admits to have left out. As a matter of fact, this is avowedly "a study of diplomatic and military history," which at times becomes a rather sketchy chronicle of British foreign policy during the French wars —the field being too broad for a medium-sized book. And is it really possible to deal properly with such a subject, while leaving economics out? Mr. Sherwig has calculated the ratio between foreign subsidies

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(£65.8 millions for the whole period) and total *British* military expenditure during the French wars; this ratio is only 8 per cent, and he concludes that foreign aid did not play a decisive part in Britain's balance of payments difficulties, which is not absolutely convincing, as he does not analyze the problems of specie supply and of speculation against sterling. He stresses the importance, in the later stages of the wars, of arms, ammunitions, and various equipment supplies by Britain to her allies (in 1813, they received nearly one million guns), but he does not tell where and how such large quantities of war materials were produced. He stresses rightly that financial help to foreign allies was made possible only thanks to Britain's expanding economy and increasing productivity, but this point would have certainly deserved more attention.

Moreover Mr. Sherwig has completely identified himself with his hero —His Majesty's government—and sees the French wars through the glasses of an old-fashioned staunch British patriot. His preface is dated "Waterloo day 1968"! Britain struggles heroically "for existence," for the European balance of power and the sanctity of treaties, without any selfish ambitions (what about the sugar islands?). There is extravagant praise of British (and Portuguese!) victories, while the French are constantly "soundly beaten." Napoleon is a "tyrant," whose "arrogant ambition" and "reckless aggression" had "too long . . . disturbed the peace of Europe." As for Britain's allies, they are a selfish, grasping, irresolute, cowardly, and rather treacherous lot, for a long time unconscious of the dreadful French danger. However, this is a minor criticism, and though it is a pity that Mr. Sherwig has stuck to an old-fashioned "diplomatic" approach, his book is nonetheless useful and valuable.

François Crouzet
The Sorbonne
Paris

France in the Age of the Scientific State *by Robert Gilpin. Princeton, Princeton University Press, 1968.—474 pp. $12.50.*

Sociologists have drawn our attention to the increasing importance of science and technology in shaping society in what Daniel Bell has called the post-industrial age. Political scientists have been slower in dealing with scientific policy either as a new field of public policy of crucial interest to any student of a political system or as an essential ingredient of national power for any nation that wants to play a major role on the

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world stage. The model of the nineteenth century nation still dominates the thinking of most specialists of comparative politics or of international relations (the sudden appearance on the stage of so many new and undeveloped nations has tended to accentuate rather than correct this example of conceptual retardation). Professor Gilpin belongs to the very small group of political scientists who have fully understood the importance of the "scientific State." His first book, *American Scientists and Nuclear Weapons Policy*, is a splendid case study which demonstrates both the importance of the scientific community in a major area of national security policy and the divisive and confusing impact of political questions and value judgments on that community. His second book is a genuinely pioneering work. It examines France's science and technology policy and raises fundamental problems.

For the student of French government concerned with the interaction of society and politics in a nation whose state has usually reflected or exaggerated social rigidities and fragmentation, except after major crises, Professor Gilpin's book provides a thorough discussion, not only of scientific and industrial policy since World War II, but also of France's nineteenth century heritage. His analysis shows that in this area as in so many others, Napoleon was both the decisive innovator and the evil genius who put France into a straitjacket of bureaucratic centralization and uniformity which has never been broken and which has, in turn, shaped (or twisted) the very far-reaching attempts at modernization undertaken since World War II. Professor Gilpin's book includes, among many other topics, a remarkably comprehensive and incisive study of France's system of higher education and thus provides a most useful background for an understanding of the university collapse in May 1968.

For the student of international relations, Professor Gilpin raises an issue of central importance: what is the meaning, what are the possibilities of "independence" for a middle power today? Both the Fourth and the Fifth Republics have tried—unsystematically in one case, loudly in the second—to provide France with the modern means of independence: not only a nuclear force, but a national technology in all sensitive areas such as space, defense, aeronautics, computers. For complex reasons, limited resources, but also the deficiencies of French bureaucracy and industrial management, the results have often been poor: France was unable to meet the "American challenge" or to match the "American model of a scientific state." Only Western Europe has the necessary scale and resources. Yet merging with its neighbors into an integrated, federalizing Europe would have deprived France of the very independence its leaders were seeking. De Gaulle's attempt at overcoming this contradiction was a policy of extensive cooperation with France's partners in EEC

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(and with Britain). As Mr. Gilpin points out, the Fifth Republic's policy was both the most nationalist and the most internationalist France had known. But it was not supra-nationalist; and as he shows in his concluding chapters, only a European entity with a clear political will and central power could overcome the countless obstacles which have thwarted de Gaulle's attempt to finesse the insufficiencies of the nation-state.

Thus Mr. Gilpin's conclusions are the same as those of Jean-Jacques Servan-Schreiber's best seller. But Professor Gilpin's book is infinitely more rewarding—not only because he is more prudent in his tone, less determined to overlook the reasons why the kind of European entity that alone could do the job is in fact resisted by the old yet resilient political, economic, and social structures of the separate nation-states, but also because of the scholarly depth and scope of his analysis. Given the past failures of the bold Gaullist enterprise and the slowness with which a genuine European state could emerge even in the most optimistic hypothesis, the Gaullist leaders of post de Gaulle France now try to find a way out in a reform of France's internal, structural obstacles to change—breaking up both the Napoleonic and the French bourgeois heritage, a task that appears at least as difficult as the making of Europe. They would benefit greatly from reading Professor Gilpin's diagnosis.

If politics in the post-industrial age will consist not of the utopian extensions of scientific rationality to politics, but of the clash between the demands of science and technology and the demands of non-scientific institutions, habits, and values; if international politics in the age of science will consist not of the triumph of interdependence over the obsolete nation-state, but of the more or less successful attempts by nation-states to manipulate interdependence, Professor Gilpin's book is a most rewarding and rich contribution to a political science that would be rigorous, readable, and "relevant."

STANLEY HOFFMANN
Harvard University

The Early Vasas: A History of Sweden, 1523-1611 *by Michael Roberts. Cambridge, Cambridge University Press, 1968.—xiv, 509 pp. $16.00.*

Any contribution to Swedish historiography in English is welcome, and a thorough scholarly work like the present one is particularly so—even

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if it can hardly be called a "History of Sweden," as the subtitle states. For that, it lacks too much of what makes up history in social, economic, military, diplomatic, intellectual, artistic respects. But what it does offer is, in a perhaps old-fashioned way, a detailed, comprehensive constitutional history. As such, it gives at least one dimension in Swedish history that is little known and suggests comparative studies to the average European historian.

Strangely enough, the main title of the book, "The Early Vasas," is likewise justified in a limited sense only. The events in the lives of the chief actors and their characters are peculiarly submerged; we hear practically nothing about the Vasas except for their political game. We learn little of Gustavus Vasa's upbringing, character, and seizure of the crown of Sweden and—notwithstanding all the book's accent on ecclesiastical history—when, how, and to what extent he became a "Protestant." Only indirectly can we deduce that he was pragmatic, cunning, able, cantankerous. With him and his sons, human motivations are seldom alluded to. Only the chapter on Erik is introduced by a fine description of this fascinating personality. John III, the author's favorite, remains again a rather vague figure and his personal relationship to Erik is as little illuminated as his conduct in his Finnish dukedom, his religious inclinations, and the role which his Catholic Polish wife Catherine played in his life. Charles IX is somewhat superficially and unconvincingly painted as a villain, yet of the three sons of Gustavus, he was politically the most gifted, and perhaps he had more sincere convictions than either his father or his brothers. Sigismund remains, justly, somewhat apart, but shares vaguely with his father John the author's sympathies.

"Religious and constitutional questions were tightly intertwined" (p. 394). This statement encompasses the author's preoccupation. In the introductory chapter, we become somewhat acquainted with social and church conditions and the financial difficulties as Gustavus Vasa found them. The accent is on the differences between Sweden and the rest of Europe. Then, parliamentary and ecclesiastical questions take over, with perhaps too much—and anachronistic—emphasis placed on "nationalism" and "parliamentarism." There follows the long struggle between king and nobility and his slow emergence as the powerful ruler at home, who understood also how to secure independence from former friends and enemies abroad. In connection with this struggle, the infinitely cautious policy with regard to religion, which avoided a clear break with Rome and yet introduced Protestantism—a feat unequalled anywhere in Europe—is carefully described and is one of the major contributions of the book. Commercial questions are but slightly (and

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not quantitatively) touched upon; diplomacy is hardly treated at all. Also under Erik, whose reign constitutes for the author a transition to some kind of "absolutism," we are led back to questions of constitutional relationships. Nothing is said about court life, art, and intellectual pursuits, which did begin to play a role in Sweden. Even Erik's foreign policy, although his special contribution, is minimized. It is hardly justifiable to see it chiefly in the light of imperialism and not in that of personal interests, internal issues, external opportunities, economic pressures, and needs for Russian friendship and for defense against a Poland which constituted a real threat through her actions in Livonia. Also later, under John III and Charles IX, the Swedish-Polish relationship is not analyzed beyond the personal role of Sigismund and the curious spectacle of the Poles being afraid that their king might not wish to be their king. The account of Charles' reign is one of a long succession of machinations at *Riksdags* and of agreements and resolutions. Military actions are mentioned, but the accent lies on the duke's cunning use of popular representations, councils of the nobility, capable secretaries, and ultimately occasional violence and clever manipulation of *Herr Omnes*. But again descriptions of political moves and institutional measures overshadow those of human psychology and of the events themselves and their consequences.

The long story is from time to time pleasantly relieved by ironical observations. Footnotes are few, but the bibliography of the Swedish material is excellent. The general European historian might have appreciated, though, more attention also to the important works in German, English, Polish, and Russian. As a whole, the book thus appears as a valuable contribution within the limits the author set for himself, and it suggests the need for extensive further work to round out the picture.

WALTHER KIRCHNER
University of Delaware

The German Officer Corps, 1890-1914 *by Martin Kitchen. Oxford, The Clarendon Press, 1968.—xxix, 242 pp. $5.50.*

Armed Forces and Society: Sociological Essays *edited by Jacques Van Doorn. The Hague and Paris, Mouton, 1968.—386 pp. 50 Guilders.*

In the first part of Hermann Broch's novel *The Sleepwalkers*, one of the characters reflects upon the meaning of the military uniform, which

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"provides its wearer with a definitive line of demarcation between his person and the world." It is, he muses, "like a hard casing against which one's personality and the world beat sharply and distinctly and are differentiated from each other, for it is the uniform's true function to manifest and ordain order in the world, to arrest the confusion and flux of life, just as it conceals whatever in the human body is soft and flowing. . . ." It would be difficult to find a passage that describes more graphically the feeling of separateness that has so often affected professional soldiers in modern industrial society and has led in many cases to a sense of alienation, a consciousness of an estrangement between their own way of life and the values of their society. In its extreme forms this has led—European history since 1815 is full of examples—to military conflict with civilian authority and to violent attempts to overthrow constitutional regimes.

The classical case of alienation and resultant civil-military friction is, of course, Germany, where the feudal ethos of the Prussian officer corps was in sharp contrast to the political and social aspirations of the middle and working classes and where the army's prestige was one of the greatest barriers to progress towards liberalism and democracy. The literature on the subject is already immense, but Martin Kitchen has made a valuable addition to it with his analysis of the political behavior of the officer corps during the reign of William II. What emerges from this well-documented and highly original study is that the military establishment suffered as keenly from a sense of encirclement by domestic foes as from one by foreign powers, and that its response to both was equally neurotic.

In his treatment of the army's views on foreign policy, Mr. Kitchen has no hesitation about arguing that the military chiefs were needlessly alarmist in their assessment of Germany's external situation and irrationally drastic in their plans to meet what they considered to be threats to the national security. But the illogic of the famous Schlieffen plan was nothing to that which informed their thinking about domestic affairs, which was continually haunted by fears of a democratic victory in Germany that would inevitably destroy the army by causing a dilution and deterioration of the political and social homogeneity of its officer corps. In a series of short but pithy chapters based on new archival materials, Mr. Kitchen describes the social structure of the officer corps and its code of honor and then goes on to analyze its attitude toward civilian society in general and the growth of socialism in particular, pointing out, among other things, that there was a distressing willingness on the part of some local army commanders to resort to naked force against movements that they considered to be subversive. The famous

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Zabern case of 1913, which Mr. Kitchen discusses in detail, was less a case of military insensitivity to local conditions than a symptom of military insubordination caused by alienation.

That these tendencies were not restricted to prewar Germany is shown in the highly instructive group of sociological essays edited by Jacques Van Doorn, which deal with the role of the military in both the advanced and the developing nations in the period since 1945. In his preface, Morris Janowitz supplies another reason for the alienation felt by the professional soldier when he explains how the impact of technology and mass destructive weapons have changed the pattern of international relations and the threat of general war and, by doing so, have radically changed the soldier's position and function. The problem is elaborated in a fine essay by M. D. Feld on "Professionalism, Nationalism and the Alienation of the Military" and by studies of the role of the officer corps in Sweden, Great Britain, the Soviet Union, and Germany and France by Bengt Abrahamsson, C. B. Otley, Roman Kolkowicz, and Kurt Lang respectively. In his provocative essay, Professor Lang gives us additional examples of ways in which soldiers have fought back against the threat of isolation and powerlessness, by comparing the abortive German military revolt of July 1944 and the Algerian *Putsch* of 1961. His conclusion is that, at least in highly developed societies, the military, while continuing to be the only group to possess the means of violence necessary for a forcible overthrow of constituted authority, suffers from "a fundamental incapacity . . . to give their revolt an effective political underpinning."

This is probably less true of the developing nations, where the military is apt to be the most progressive political force and to be less bound down by the burden of their own tradition than armies in older societies. In general, this is the view proposed in M. J. V. Bell's essay on the African states, Khatid Sayeed's analysis of the role of the military in Pakistan, and Eugene Kim's account of the South Korean military coup of May 1961. But for the older military establishments the problem of alienation remains and may, as Professor Feld writes, be insoluble. In his view, "except for the extremity of total war and for the two or three states for whom this macabre condition represents a political alternative, the professional soldier has been reduced to a role which the value structure of his society categorizes as being both marginal and dysfunctional. In an incrementally-minded situation he does not appear to have any positive contribution to make."

Not all the authors in this volume would take so stern a line. The military contribution may have to be redefined—perhaps along the lines of Janowitz's constabulary force concept—but that it will be needed un-

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til the world is considerably more peaceful than at present seems obvious, and this is underlined in J. A. Jackson's thoughtful essay on "The Irish Army and the Constabulary Concept" and Van Doorn's terminal essay (with Jan H. Mans) on United Nations forces and the legitimacy and effectiveness of international military operations.

GORDON A. CRAIG
Stanford University

The Conspiracy against Hitler in the Twilight War: An Account of the German Anti-Nazi Plot from September 1939 to May 1940 and the Role of Pope Pius XII *by Harold C. Deutsch. Minneapolis, University of Minnesota Press, 1968—x, 394 pp. $8.95.*

Professor Deutsch believes that a true appreciation of the importance of the German opposition to Hitler has suffered from many handicaps, including an unwitting alliance between anti-Germans, new-Nazis, and the many ordinary Germans who resent the heroism of the resistance because of the shame it reflects on their own supine behavior. There may be some truth in this theory, but surely the most serious difficulty in studying the resistance is the absence of reliable source material. Conspiratorial documents are presumably scarce to begin with, and the bulk of the evidence that fell into the hands of the Gestapo and other Nazi agencies appears to have been destroyed. Documents on contacts with the German resistance that may exist in foreign archives have not yet been made available to researchers. Why not, one wonders. Surely the Vatican could reply most effectively to attacks on Pope Pius XII by revealing his part in the conspiracy against Hitler. Whatever the reason, Professor Deutsch has been forced to rely heavily on memoir evidence, written and oral. His sympathetic treatment of the anti-Nazi leaders and the difficulties with which they had to contend is a welcome contrast to the smug skepticism of their critics, but the nature of his evidence inevitably casts doubts upon its accuracy. Human beings tend to recall or reconstruct a past which puts themselves and their friends in a favorable light.

Professor Deutsch believes there were four major phases, or rounds, of German resistance activity. The first, set off by the Blomberg-Fritsch crisis and Hitler's reorganization of the German armed forces, ended with the Anglo-French surrender at Munich. Round two, from the beginning of the war to May 1940, ended with Hitler's victories in the west. Round three, from 1942 through 1943, was an effort to take advantage of German defeats on the eastern front. And finally, round four, the most fam-

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ous of all, ended with the unsuccessful assassination attempt of July 20, 1944.

During round two, the subject of the present study, the German opposition appears to have been primarily concerned with getting in touch with Allied governments, especially the British, to secure assurance of support. To convince these governments of their sincerity and of the high-level nature of their positions in Germany, the conspirators attempted to work through channels that seemed to be above suspicion, such as the Vatican, and they revealed political and military secrets that could only be known to persons of the highest rank. Professor Deutsch's descriptions of the members of the opposition and their activities is fascinating, but the big question remains how a conspiracy that included so many high-level army leaders and other officials in key positions could have failed so completely. Professor Deutsch does not specifically raise this question, but his book provides a number of answers. The conspirators lacked decisive leadership, they lacked cohesion, they lacked luck, but above all they lacked a sense of reality. How could they expect Allied leaders to believe in the clandestine information reaching them from Germany, and what did they hope to accomplish through the communication of such information? Only the arrest or assassination of Hitler and the takeover of the German government by the conspirators could have convinced the Allied governments of their seriousness of purpose and provided the basis for fruitful negotiation. Professor Deutsch insists that "to eliminate the regime was always the principal preoccupation of all Opposition sectors" (p. 101), but the evidence he has assembled does not bear out this statement. In fact, the chief weakness of his book is his failure to provide a clear definition of the methods and ultimate aims of the conspirators—a lack of definition that appears to have been the major weakness of the resistance movement itself.

NORMAN RICH
Brown University

Finland, Germany and the Soviet Union, 1940-1941: The Petsamo Dispute *by H. Peter Krosby. Madison, Milwaukee, and London, The University of Wisconsin Press, 1968.—xiii, 276 pp. $6.75.*

The Nazis burnt down most of Europe but never a document, and the vast German archives continue to provide scholars with material for books on the diplomatic and military history of the Second World War

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and related subjects. Professor Krosby has delved deeply into this treasure-trove. He set out to look for the causes of Finland's involvment at Germany's side in the war against the Soviet Union, and he stumbled on the Petsamo question. He found that more than half of the German documents on relations with Finland in 1940-41 dealt with the nickel deposits in that northernmost corner of Finland on the Arctic Sea.

For about twelve months after the end of the Winter War between Finland and the Soviet Union and until the German invasion of Russia in June 1941, Petsamo was the focus of a four-cornered diplomatic contest. The Germans regarded the nickel ore that could be obtained from Petsamo as vital for their war effort and were determined to secure it for themselves. The British were anxious to stop them from having it. The Soviet interest in Petsamo went beyond the economic aspect: Moscow was concerned with the strategic importance of the area and wished to assert its own predominance over Finland as a whole. The Finnish government used Petsamo as bait to enlist German political support against Soviet pressure. Thus the Petsamo question, combining as it did political, economic, and military factors of prime importance, acted as a remarkably accurate barometer of Finnish-German relations in general.

Professor Krosby has woven the many different strands of this complex affair into a fast-moving story. Inevitably the diplomatic play around Petsamo is seen largely through German eyes. But the author is aware of the bias inherent in his material and has taken pains to correct it. The Soviet archives of course remain closed, but he has done all that is humanly possible to get the Finnish side of the story. (He has not had access to official Finnish archives but this is hardly a serious loss, for Finnish officials lack the German mania for putting everything down on paper.) Finland's position in the cross-fire of big-power interests was indeed "unenviable," to borrow a phrase from the publisher's blurb. Yet the Finns did not do so badly in the Petsamo affair. They successfully used the delaying tactics in their talks with the Soviet Union, and as soon as German support for Finland against Russia had been assured, they used the same tactics to keep Germany at arm's length. Finland was nobody's satellite.

Could Finland have stayed out of the war between Germany and the Soviet Union? Professor Krosby says no. He supplies evidence to show that in case Finland had refused to cooperate with Germany and had tried to maintain a neutral position, the Germans would have seized Petsamo and probably all of northern Finland. No doubt the Soviet Union would have retaliated. Finland would have become a battlefield

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between the two giants. Actually Finland retained control over her own affairs. The country was never occupied. This surely is the key to Finland's continued independence.

MAX JAKOBSON
The Permanent Mission of Finland to the United Nations

Planning and Productivity under Soviet Socialism *by Abram Bergson. New York, Columbia University Press, 1968.—95 pp. $4.00.*

Although no anniversary of a major historical event has prompted such an outpouring of scholarly works as did the fifth anniversary of the Bolshevik revolution in 1957, few of them seriously inquire into the success of what has been a major goal, surely second to none, of the Soviet regime during the five decades: namely, the rapid industrialization of the country and the raising of its economic efficiency to levels above those of the leading countries of the capitalist world. It is primarily this question of "economic merit" to which Professor Abram Bergson addressed himself in a series of three lectures delivered at the Carnegie-Mellon University, and which are printed—together with a brief technical appendix—in the slim volume under review.

By *economic merit* Bergson means two things, distributive equity and productive efficiency, but concerns himself here only with the latter. *Efficiency* he further subdivides into *static efficiency*, which pertains to the utilization of its resources and technological possibilities by an economy at one point in time, and *dynamic efficiency*, which refers to the degree to which the growth in output outstrips the growth of inputs. The core of the book consists of several tables which present Bergson's findings on this score, for the year 1960 in the case of static efficiency, and for the period 1950-64 in the case of dynamic efficiency. For the USSR, his calculations rest on his own well-known and extremely careful estimates of the growth and structure of Soviet national income; for the United States and Western Europe, Bergson relies on the findings of other investigators, especially Edward F. Denison. No one knows better than the author himself the innumerable conceptual complexities and statistical pitfalls of such comparative calculations. Bearing this in mind, we may note the following results. According to the author, in 1960 Soviet real national income per employed worker (adjusted for labor

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quality) was on the Italian level, was 29 to 48 per cent of the American (depending on the weights used in constructing the underlying index numbers), and was 60 to 80 per cent of that in the leading Western European industrial powers. If national income is related to a composite of both labor and reproducible capital, Soviet static efficiency is still approximately on the Italian level, is 34 to 56 per cent of the American level, and is 60 to 85 per cent of that in the other leading Western European countries. As for dynamic efficiency, Bergson finds that in the 1950s and early 1960s the Soviet real national income per worker grew faster than in some but slower than in other leading capitalist countries and did even slightly less well in relation to both labor and capital taken together. This leads the author to conclude—in his own terms, correctly—that the performance of Soviet socialism in recent years compares, but not especially favorably, with the performance of the major capitalist countries. A good part of the little book is hence devoted to the sources of inefficiency in the Soviet economic system—a very convenient summary of the studies of various Western scholars, including Bergson himself.

It should be noted that the author's main concern is with *efficiency* and that dynamic efficiency is not the same thing as growth. In regard to growth, the Soviet economy has done better than the Western countries considered by Bergson (as we know from his own massive research) largely a result of putting a larger part of its resources into growth-inducing activities, especially into investment. Thus, the Soviet capital stock grew at the remarkable rate of about 10 per cent per year between 1950 and 1962. It is in part because of the extraordinarily fast expansion of the supply of inputs that its dynamic efficiency has been so moderate. Yet might we not ask whether it was growth as such, apart from dynamic efficiency, that concerned chiefly the Soviet leadership in recent decades from the standpoint of its domestic and international objectives and commitments? The author recognizes this point (p. 17) but does not pursue it. And might not the preoccupation with rapid growth itself—like the general's preoccupation with winning a battle—have occasioned the neglect of dynamic efficiency, that is of the efficiency of the growth process. The author does point out that the low *static* efficiency of the Soviet economy had deprived it of resources that might have also been devoted to growth. True, and what is more, the glaring shortfall in *static* efficiency may themselves be traced to the peculiarities of the Soviet economic institutions, which have been designed primarily to mobilize resources and to expand production at maximal rates, regardless of cost and which have therefore seriously impaired the efficiency of resources

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use at any one point in time. It is the realities of the shortcomings of this "extensive" form of industrialization that lies behind much of the pressure for reform in economic institutions in the USSR and other countries of Eastern Europe.

GREGORY GROSSMAN
University of California, Berkeley

Labor in American Politics *by J. David Greenstone. New York, Alfred A. Knopf, 1969.—viii, 408 pp. $8.50.*

This excellent study is concerned not with the voting behavior of the rank-and-file and their families but with how unions have attempted to organize campaign work, how leaders recruit members for political activities, and what it is that they actually do once recruited.

Gompers' A.F.L. strategy, unlike that of the Knights of Labor, was to appeal to a narrow craft constituency; politically he was not interested in coalitions with non-labor groups. The tactic was to reward labor's friends and punish its enemies, on a nonpartisan basis. As one of his associates put it: "We have no ultimate ends, we are going on from day to day. . . . We are practical men." Even so the A.F.L. was never all that practical or all that nonpartisan. The leadership began to show a preference for the Democratic party as early as 1906, and the membership had come to share that preference by the 1920s. The "critical" election of 1928 set the stage for the active alliance between organized labor and the Democratic party which was forged in the mid-1930s. Since then organized labor has been an integral part of the Democratic party, a party within that party. It has provided the Democrats with organizational muscle, a cadre of trained political specialists, and perhaps a fourth or more of its campaign funds. The alliance has been stronger at the national than at the state or local levels and has worked better with factory than with non-factory workers. What the unions have been able to do is to help the party increase the size of its urban majorities, often by working in low-income, predominantly Democratic precincts in which, ironically, union membership has not been large. At the local level, in some communities, non-factory workers, notably the building trades unions, have continued to play the old Gompers game with some success, but, in general, voluntarism as a laissez-faire political doctrine was dead by the end of World War II. The emergence, during the New

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Deal, of organized labor as a major, nation-wide electoral organization within the Democratic party represents, in Greenstone's view, the most important change in the structure of the American party system during the last quarter century.

Greenstone chose wisely in selecting Chicago, Detroit, and Los Angeles for close-range studies of how labor action in politics is affected by environmental variables. In Chicago, a tightly controlled Democratic machine has kept organized labor in the role of a mendicant, seeking favors, helping the machine, but never accepted as part of it. In Detroit —in Michigan in general—the U.A.W. had little difficulty in acquiring a dominant position in the Democratic party, thanks in part to state legislation dating from the Progressive period which facilitated rank-and-file participation in political parties. But Detroit's nonpartisan elections, a heritage from the same tradition, combined with latent anti-Negro sentiment, has kept labor at some disadvantage in Wayne County. In Los Angeles, weak party structures, the absence of a single large industry as in Michigan, a high degree of social disorganization occasioned in part by continuous heavy in-migration, and massive residential mobility, have created special problems for organized labor's participation in Democratic party politics.

In a concluding chapter on "Labor Politics in a Fully Industrialized Society," Greenstone advances a useful and significant theory of the political role of labor in today's society. Using insights from Dahrendorf and Durkheim to update Marx, he argues that class is still a useful tool in analyzing how social change takes place. But today the essential class division is not between those who own the means of production and a general, undifferentiated "proletariat," but between what he calls economic authorities and economic non-authorities. Stated another way, authority relations have superseded property relations as a determinant of class conflict. The class designated as "economic authorities" includes all those with responsibility for, and decision-making authority over, the process of production. Economic non-authorities include both wage earners and those with no role at all in economic production. The line of demarcation is between those—regardless of the size of their income —who have a functional role in production decision-making and those who do not. The former feel that they have a stake in rationalizing production, regardless of the immediate social consequences, in order to maximize production, efficiency, and profits. In the end, rationalization may minimize conflict by creating an ever larger economic "pie" but, in the short term, it can visit harsh and often unnecessary hardships on sections of the population. In a word, instrumental rationalization of

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production generates the issues about which class conflict centers in a fully industrialized society.

By the 1960s, a new consumer interest had emerged which, in theory, provides a basis for unifying the economic non-authorities for political action. This common interest is to be found in such matters as: specific consumer interests in the market (labeling, purity and safety and price of products, rates of interest, and the like); the exclusion of certain groups from full and active roles as consumers; the attempt by the economic authorities to impose the social costs of rationalization (including a major share of the tax burden) on the economic non-authorities; and the need to create, preserve, and restore collectively consumed goods, tangible and intangible, cultural, recreational, and educational by taxing the economic authorities in order to achieve these ends, thereby reducing profits and perhaps limiting output.

Given the organizational weakness of the Democratic party, organized labor might be regarded as the logical organization to defend and advance the new consumer interest. But—this is where Durkheim enters—the division of labor has created economic disunity in the ranks of labor. Then, too, while members of trade unions have little to say about production decisions, they have a direct stake in production. Union members *perceive* their economic interests much more clearly than they perceive their class interests as consumers, i.e., economic non-authorities. They will support long, bitter, and costly strikes but show little enthusiasm—much less in fact than middle-class liberals—for active political work. The consumer interest is latent and diffuse. The stake in a job is clear and immediate. The harder labor leaders press the consumer class interest, the less actively the membership responds. Thus, in one sense, the development of specialized skill groups has reinforced the old bread-and-butter conception of political action. For this reason, the leadership prefers to operate within a broad coalition, such as the Democratic party, rather than act independently.

Greenstone's theoretical argument, in the concluding section, would have been strengthened if he had weighed two closely related considerations. He refers to defense contracts at one point but merely in passing (p. 390). Yet over the period covered by this study, from the New Deal to the mid-1960s, a high level of employment and production has been maintained by consistently heavy reliance on defense spending. Organized labor has played a key political role in supporting the policies which have led to augmented war production. By supporting these policies it has, from a long-term point of view, undercut the consumer interest of its membership. The formation of the new working alliance

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between the Teamsters and the U.A.W. is based, to some extent, on a belated recognition that national priorities need to be reordered. In a sense one might say that organized labor became an active partner in the military-industrial complex because of its alliance with the Democratic party. But there is another aspect of the matter that would also have warranted discussion. Organized labor's self-conducted purge during the period from 1948 to 1954 set the stage for the leadership's ability to align the entire labor movement in support of cold-war policies.

This most perceptive study, based on first-rate original research, should be required reading for all those concerned with such critically important issues as: What is the political role of labor in a fully industrialized society? Is organized labor finished as a significant agency for social change? How—under what circumstances—could a revitalized labor movement once again become the spearhead of social change?

CAREY MCWILLIAMS
New York City

Canadian Labour in Politics *by Gad Horowitz. Toronto, University of Toronto Press, 1968.—271 pp. $7.50 (Canadian).*

This fine book presents the relationship between organized labor and political parties in Canada and specifically the ebb and flow of trade union support for a socialist party, the Co-operative Commonwealth Federation (CCF), and its successor, the New Democratic Party (NDP). If the standard of comparison for Canadian politics is the United States, the question is, why should there be a socialist party in Canada? But given one, why is it, in contrast to the British Labour Party, so weak in comparison with the middle-of-the-road Liberal Party? In a brilliant essay at the outset of his book, Horowitz answers by taking off from Louis Hartz's thesis on fragment societies. As new societies were detached from their European origins, they were left with incomplete political ideologies. Armed with a fragmented political culture, their development was inevitably different fom the original founding society. This argument is used by Hartz to explain the differences between the United States and Canada, on the one hand, and Britain on the other. In addition, because of the revolutionary experience in the United States coupled with the emigration of potential counter-revolutionaries to Canada, some crucial aspects of political ideologies were from then on absent in the United

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States but present in Canada. The most relevant element found in Canada but missing in the neighboring country is the "tory touch." It is this tory heritage which provides the context for the emergence of socialism, both because of historical dialect and the similar tory and socialist views of society: corporatist, organicist, and collectivist. Egalitarianism is a principal component of liberalism and it, in combination with the tory view, gives socialism its unique perspective. A society missing one of these ideologies as a legitimate part of the political culture is consequently limited in the kinds of political parties it can develop.

It is possible to utilize the general Hartzian approach to also incorporate the place of political ideologies in French Canada, and to some extent Kenneth MacRae in Hartz's *The Founding of New Societies* does so. Horowitz, however, pays little attention to the French Canadian situation. There is good reason for this, since trade unions have faced unique problems in Quebec and followed unique paths of development. But the omission of this area still leaves incomplete the history of Canadian labor.

If the notion of toryism is a provocative theme in explaining the existence of socialism in Canada, it seems much less successful in accounting for the relations between organized labor and a socialist party. For example, Horowitz attributes the lack of strong trade union support for the CCF in the early 1940s to four basic reasons, one being the tactics of the CCF itself in the face of its unusual growth and popularity at that time. Other reasons were the number of locals dominated by Communists who opposed affiliation with the CCF, the example of the American CIO unions' approach to party politics, and the relation between the TLC craft unions and the Liberal Party. In other words, nothing in the earlier formulations contributes to an explanation of the weak connection between organized labor and the CCF.

Horowitz's sympathies are clearly with the NDP, although generally this is not intrusive. The reader should be wary, however, when in discussing the struggles between the CCF and the Communists for union control, Horowitz notes instances when the Liberals invited and welcomed electoral support from known Communists. Some of the events now appear incredible, but this is primarily a matter of hindsight. If the Liberals seemed much less sensitive than the CCF to the threat from the Communists, it is not so much that the former were "dupes," but rather, unlike the CCF, that they were not in any danger of being taken over, and hence could flirt with dangerous partners.

The author makes clear that the future of socialism in Canada is an uncertain one, although strongly supported by a small minority. Proportionately, the minority may be no greater than is socialist support in the United States. But the difference lies in the existence in Canada of a

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viable political vehicle, the NDP. How long can it remain a minority party and retain its viability? Canadian politics have, since the 1930s, provided a relatively stable place for third parties, though the fortunes of the parties themselves have fluctuated wildly. The NDP could remain as such a party, buffeted by events, and possibly reappearing in a slightly altered form after some particularly strong onslaughts. Whatever happens, Horowitz argues that it could not amount to much electorally without support from organized labor. Yet organized labor itself faces problems of public acceptance. Horowitz suggests an improved image for labor, thereby giving greater electoral appeal to the NDP. But it seems unlikely that a good public relations job would be sufficient to overcome the problems of labor. More reasonable is his contention that what the NDP needs to propel it into major party status is a social upheaval that disrupts customary voting patterns and leads to a search for alternative political solutions.

MILDRED A. SCHWARTZ
University of Illinois at Chicago Circle

The Brain Drain *edited by Walter Adams, preface by Paul H. Douglas. New York, The Macmillan Co. and London, Collier-Macmillan, Ltd., 1968.—xiii, 273 pp., $6.95.*

This book is a compilation of fourteen papers prepared by scholars, primarily economists, from Europe, Asia, Africa, Latin America, and the United States who participated in an international conference on the "brain drain" in August 1967. It is the best single introduction to the subject and presents a wide spectrum of viewpoints, sharply delineated, in a readable prose.

The collection is divided into five parts. The first seeks to frame the problem. It contains a charming historical sketch by Stevan Dedijer of Lund, Sweden of national policy and the migration of scientists in antiquity and in early modern times when European universities were first being organized. Alternative analytical approaches, internationalist and nationalistic, comprise Part II. Harry G. Johnson of the Universities of London and Chicago argues strongly that the flow of human capital can be subsumed within the economics of international trade with only the one addendum that compensation should be paid the developing countries who lose trained manpower on whom they have expended scarce re-

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sources. Don Patinkin of Jerusalem and Kenneth Boulding of Colorado are not persuaded that the internationalist's model is apposite. The world is not one capitalistic market, they argue, and Patinkin warns that a "productivity-ethic" is insensitive to income distribution and other social welfare considerations that must be included in a valid framework.

"Education and Migration" is the title of Part III, which contains only one chapter, by Charles P. Kindleberger of M.I.T. If this well conceived and well edited book is unbalanced, it is because of its failure to deal at greater length with the interrelations between study abroad and migration. There are other dimensions to the brain drain, such as the ease of migration of physicians and first-class academicians, but the nub of the problem stems from the large-scale flow of students abroad who, after years of separation from their homelands, find it difficult to return.

Six case studies comprise Part IV. The chapters of V. M. Dandekar of Poona, India and Robert Mossé of Grenoble, France are replete with insights and wisdom. Mossé explains why French academicians have no incentive to migrate to the United States and Dandekar reports that the malutilization of talent in India reflects the untoward consequences of poverty reinforced by the structure inherited from the days of the British which rewarded those at the top too highly and tolerated too wide a spread between the senior chief and juniors below.

The final Part, "An Agenda for Action," by Walter Adams and Joel Dirlam, is full of good sense and balance. Focus is on the real issues; the spurious ones are ignored.

Although we do not find consensus where none exists, it is stretching the facts only slightly to contend that all of the contributors agree that the brain drain stems from faulty policies in the developing countries, particularly in the structuring of their systems of higher education, and in the efforts of developed countries to find an easy way out for coping with periodic or chronic disequilibria in the demand for and supply of professional manpower.

This reviewer believes that a brain drain problem *does* exist when U. S. employers, profit and nonprofit, actively and aggressively recruit abroad for scientists, engineers, and physicians; that U. S. immigration policy, which grants quota preference to professionals and thus facilitates the migration of tens of thousands of trained men and women annually from resource-poor Asian countries, is ill-advised; and that large-scale American support for foreign students to attend colleges and universities in the U. S., a practice which encourages their permanent emigration, is a serious disservice to many struggling nations.

A defense can be made, of course, for developed nations, like the

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United States, pursuing policies such as international recruiting, quota preferences for professionals, and scholarship and fellowship assistance for foreign students. But only model builders who are willing to ignore reality will overlook the fact that free competition for talent in a world of nations with grossly unequal bargaining power will often have pernicious rather than beneficial consequences for the weak.

The wealthy can ascribe the poverty of most nations to the latters' errors of commission and omission and plunge ahead to realize their own objectives or they can assume some responsibility for the situation and act accordingly. A brain drain exists. All that is in question is how the wealthy should act.

ELI GINZBERG
Columbia University

Science and the Federal Patron *by Michael D. Reagan. New York, Oxford University Press, 1969.—vi, 346 pp. $7.50.*

The complex set of relationships that has evolved since the Second World War between the scientific community and the federal government over the support of research is sketched and probed lucidly, sympathetically, and economically in this fine book by Michael D. Reagan, a political scientist at the Riverside campus of the University of California. The scientific community needs money for its increasingly costly research; the federal government needs—what? It is primarily this ambiguity in motivation for the support of research that lies at the heart of the problem.

The federal government at this point in our history exhibits little hesitation in establishing priorities for expenditures on applied science, which account for 85 to 90 per cent of the 16 billion dollars now spent annually on research and development; we seem mainly to require weapons, weapon systems, and space hardware as evidenced by the fact that the lion's share of development funds is spent by the Department of Defense, the National Aeronautics and Space Agency, and the Atomic Energy Commission. The hesitation and ambiguities appear in the support of basic research, where the one agency charged specifically with nurturing basic research in this country, the National Science Foundation, administers only 11 per cent of the 15 per cent of the federal R and D budget that goes to basic research; the remaining 89 per cent comes from the agencies listed above as well as from the Department of Health, Educa-

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tion, and Welfare. Reagan presents and analyzes the spectrum of answers to the question of how, if at all, these agencies, charged statutorily by the Congress with rather well-defined missions should go about supporting basic research.

The analysis covers the gamut from the administrational organization within the government to the philosophical underpinning for the support of basic research by the taxpayer. It follows the evolution of ideas and practices from the beginning by the Office of Naval Research at the end of the Second World War to the present multi-agency panoply. As a scientist, I must express my admiration for the author's grasp of the style and ambiance of the politics and practice of basic research. His picture of how decisions are made, or not made, and carried out, or not carried out, rings true.

Professor Reagan is not satisfied with only providing a first-rate summary and analysis of the answers given by others to this problem; he bravely strides in and presents his own suggestions on the list of priorities that should govern the allocation of funds for basic research. His unassailable criterion is "social benefit," which must include the proposition that "cultural enrichment of the community through greater understanding of the universe is as much a social benefit as the advance of the economy." Considerations of immediate advantage win out, though, and his assailable conclusions are that first priority be given to basic research that is most likely to contribute to "those social objectives which are defined as most urgent politically"; second priority goes to science-related educational needs at all levels; third priority goes to undirected small-scale research; and fourth priority goes to undirected research in those fields requiring unusually expensive equipment or large facilities. Since the federal government provides about three-fourths of the funds spent by universities on basic research, this list of priorities would impinge immediately on the intellectual choices of university professors in science departments. Further, this order of priorities would presumably also apply to a National Foundation for the Social Sciences that is so persuasively championed by Reagan. A thorough examination of the possible undesirable consequences of utilizing the power of the purse by the federal government to turn the research in universities toward "social objectives which are defined as most urgent politically" is, unfortunately, not included in this book. Past experience with federal support of the physical and biological sciences immediately suggests two hazards that would be likely to ensue and, indeed, already exist to some extent: pressure for political conformity by the recipients of federal support and a less congenial context for the innovative research that opens new fields

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rather than cultivates more assiduously those that are already known. Our understanding of these as well as other more subtle problems attendant upon federal support of research could profit from an analysis at the same high level as that exhibited by Professor Reagan in those areas of this problem that he did choose to probe.

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Looking at History Through Mathematics *by Nicholas Rashevsky. Cambridge and London, M.I.T. Press, 1968.—xviii, 199 pp. $10.00.*

In the last decade many historians have started to use quantitative techniques in their research. For the most part they have not advanced much beyond the counting stage, probably because of their lack of statistical expertise. Any book that demonstrates how statistical or mathematical models can be used to describe and explain historical configurations is bound to stimulate deeper work by historians and also help other social scientists extend their theories along the time dimension. This small, excessively expensive book by a foremost biomathematician carries a promising title, but fails in its mission.

Essentially Rashevsky has developed a family of diffusion models applicable to questions about the distribution of beliefs, trade patterns, mass behavior, and social structure. The models are neither especially convincing nor especially useful. In the first place his mathematical toolbox is too small. The last chapter mentions non-quantitative mathematics, yet no use is made of geometry, sets, clusters, graphs, or topology. The only techniques used are probability and differential calculus, despite the fact that many of the models can be more elegantly phrased in terms of matrix algebra and Markov chains. The failure to use these methods apparently stems from an unfamiliarity with recent mathematical work in geography, epidemiology, and demography. Sophisticated epidemiological models of the diffusion of rumors and beliefs and demographic models of population change exist and are relevant to the problems Rashevsky discusses, but nowhere are they even mentioned. Evidently Rashevsky's familiarity with modern behavioral science goes no further than psychophysics, which is used extensively. Most astonishing of all, practically no use or mention is made of econometrics or mathematical statistics generally.

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The greatest failure is that none of the models are attuned to the limitations and possibilities of historical data or to the theoretical concerns of social science. Rashevsky cites Woytinski, Sorokin, the *World Almanac*, and the German *Statistisches Jahrbuch* with equal confidence. He does not know enough about how numbers are born to emphasize parameters that can be operationalized from historical sources. The few times actual numbers appear, they are either drawn from intuition (primitive hunting bands sight each other one kilometer away) or drawn indiscriminately from popular or scholarly books (these primitive bands wander 100 kilometers per year [p. 14]). Rashevsky has drawn his history from Tolstoy and Toynbee—profound sources, perhaps, but rather irrelevant ones for today's historian. One of Rashevsky's main concerns is to estimate how fast civilization developed from an abject belief in priestcraft to a freethinking, "rational" outlook, all of which smacks more of the village atheist than the modern social scientist.

In every chapter, practically every third page, the author apologizes that his models are "appallingly crude," oversimple, and nothing more than "possible" ways of looking at history. His real trouble lies not in simplicity but in intellectual isolation. Rashevsky's ingenious mathematics could well generate useful results if only he would begin collaboration with social scientists who could channel his needed talents toward live problems.

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Community Structure and Decision-Making: Comparative Analyses *edited by Terry N. Clark. San Francisco, Chandler Publishing Co., 1968.—xi, 498 pp. $7.50.*

The political power structure of American communities has been the subject of at least 150 studies. Non-American municipalities have also been studied, though not as extensively. The list of worthwhile accounts of national power elites must number at least 100. All of this work has been done in the last forty years and most of it after World War II. Aside from the empirical tradition dominated by American researchers, there is a healthy tradition of concern with power in social philosophy and in political theory. Frequently, the best minds of an age, beginning with Plato, have made significant contributions to the corpus of theory about

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social power. *Community Structure and Decision-Making* adds to this literature six essays by Clark, the editor, and fifteen studies by others, some of which have been published before. What contribution can a few more essays hope to make? A great deal. For the striking fact about the study of political and social power is that despite the outpouring of scholarly effort there has been a dearth of adequate conceptualization and a lack of empirically grounded propositions. If modern social science can contribute anything, it is in combining conceptions of power with methods adequate to deal with these conceptions.

To begin with, the problems of the field are not likely to be solved by new, even more ingenious definitions, though a number of the contributors to this volume do offer their own versions of the concept of power. Clark, for example, defines power as "the potential ability of an actor or actors to select, to change, and to attain the goals of a social system." Walton suggests that power is "the capacity to mobilize resources for the accomplishments of intended effects with recourse to some type of sanction[s] to encourage compliance." And Nuttall, Scheuch, and Gordon imply that power is the direct use of resources to sanction others so as to influence the outcome of decision-making. But the formal definition of power or influence turns out to be much less important in the studies reported in this volume than the way the concept is specified. For power is what Hemple has elsewhere called a "disposition concept," that is, a concept whose meaning becomes apparent only when it is applied in a series of concrete situations or empirically verifiable propositions. The outstanding sense of the use of power as it comes through in almost all the studies in this volume is that power, as Clark puts it, "is not a characteristic of an individual actor," nor even "a property relating actors" but rather it is a "system-relevant property." Some years ago social scientists achieved a major breakthrough by showing that good leadership was mainly not a question of proper individual character but rather much dependent on the social structure of the situation in which leadership was to be exercised. Similarly, political science will be enriched by the systematic demonstration by the authors of studies in this volume that power can be understood only in its community setting.

An example of this system approach is given by Nuttall, Scheuch, and Gordon in a very important article, "On the Structure of Influence." They resolve much of the controversy over reputational power elites simply by suggesting that influence consists of both perceived and actual access to resources which allow an actor to sanction others. Thus three types of influentials are generated: those who have both reputed and actual influence and those who command either reputation or actual in-

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fluence but not both. The authors then generate a number of propositions specifying the conditions under which one or another type will prevail. Similarly, in a study of eighteen communities Gamson shows that reputed influence is indeed a power resource, but that its effect depends on the activity and unity of influentials.

Perhaps the most important consequence of the system approach is that it demands that the system itself become a variable. And that is why the study of local communities—essentially focused on relatively trivial decision-making with limited scope—is so crucial. For only in local communities are there available a large number of relatively easily studied systems of political power. Moreover, by considering localities within different national systems, the comparative dimension can be much expanded and further, the nature of the propositions about power can be advanced beyond the trivial, or at least so it is hoped. Clark advances thirty-eight propositions about communities. Some such as "The larger the number of inhabitants in a community, the more socially heterogeneous its population" are either wrong or uninteresting. Others such as "The greater the number of effective competing political parties (or factions within a single party in a one-party community), the more decentralized the decision-making structure," border on the tautological. These empirical generalizations are not systematically linked together to constitute a theory. Nonetheless, as some recent work by Clark not reported in this volume suggests, the comparative approach which confronts even simple propositions with a coherent body of data may well yield impressive results.

A sample of the findings of the various studies reported in this volume give some flavor of the current status of the community decision-making field. Recently there have been several reviews of the vast published literature of the field in an effort to draw some generalizations. One controversy has to do with who, if anyone, runs this town. Both Gilbert and Walton reviewing the published literature in separate and quite different approaches suggest that, unlike what might be expected from common sense, urbanization on the one hand and a growing integration of local communities into national structures make for a generally more decentralized decision-making structure. Another controversy has to do with the role of values and style in determining patterns of decision-making. Wolfinger and Field for example attack the Banfield-Wilson dichotomy of "public versus private regardingness" as simply not in accord with the facts. If this difference in ethos is not important, others do seem to have consequences. Williams suggests that metropolitan areas tend to develop separate communities, in part as a response to

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differential life styles, despite all attempts by planners to integrate the metropolis.

Data from many of the studies suggest that history, country, and region still account for much of the variance in patterns of community decision-making.

Some readers may find the volume pretentious and disappointing. Others may share the editor's obvious optimism and enthusiasm. The collection mirrors both tendencies of the community decision-making field and as such serves as an excellent introduction to the subject. This reader remains optimistic that the efforts signalized by this volume may yet find Plato and Aristotle in much the same position on the subject of social power as they are now on the subject of physical power—brilliant in view of the problems and data at hand, but irrelevant in view of current knowledge.

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